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A QUANTITATIVE CONSIDERATION OF THE PURKINJE PHENOMENON

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It is well known that the relative brightness of the various colors of the spectrum at constant energy, which is fundamental in all photometric work, varies with the brightness of the stimulus light to which the eye is adapted. The following is an attempt to derive from the available data (mainly from an experiment of König's)[1] the law of this variation, so that it will be possible to correct for the varying brightnesses under which experiments on color have been made. Success has been far from complete, the available authorities being far from consistent and subject to various sources of error; but it is believed that the coefficients given below approximate the true values.

If we adopt a photochemical explanation of the sensitiveness of the eye to brightness, as is generally done, the curve of brightness should be the sum of a larger or smaller number of curves, severally representing the photochemical activity (and presumably the absorption) of the various sensitive substances in the eye. These curves should each be of constant form and position,[2] but

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[1] A. König, *Collected Papers*, 1903, 144-213.

[2] It is well known that the absorption bands of pigment solutions tend to change in form with increasing concentration, generally widening, and often unsymmetrically; but in the present case, if we assume (as is generally done) that adaptation is a matter of the decomposition of the pigment by light, and that the amount of undecomposed pigment is in inverse proportion to the illumination to which the retina is adapted, the actual concentration with usual lighting must be very small. With such concentrations the change of absorption-curve with concentration can be neglected. Only in the actual threshold case may it be necessary to consider the matter. The most frequent effect of increasing concentration seems to be to flatten the apex of the absorption curve, and this effect can perhaps be seen in some curves of the absorption of the visual purple, which was so concentrated that on occasion more than half of the light of the maximal wave-lengths was absorbed.

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should vary in relative height with the changing adaptation of the eye. The law of their variation should be represented by a smooth curve, and if it is true that adaptation is due to bleaching of the photochemical substance, the surviving amount and therefore the sensitivity of the retina should vary inversely as the luminosity; except in the case when the amount of effective light is minute and the restoration of the bleached substance can therefore be viewed as immediate. In that case the amount bleached will vary as the luminosity, so that the amount in function will vary as

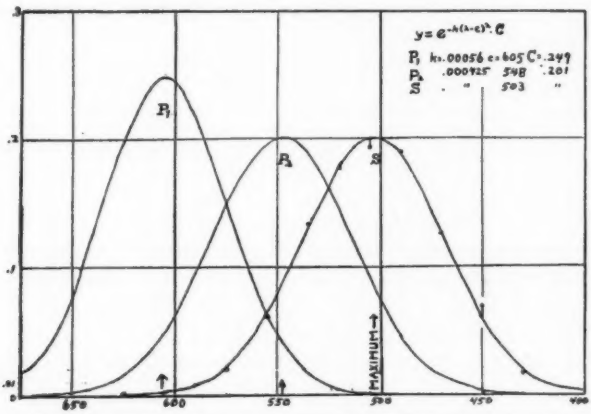

FIG. 1. THE THREE ELEMENTARY PROCESSES, PLOTTED TO APPROXIMATELY EQUAL AREAS

The dots represent the points of König's S curve, reduced to normal spectrum and sunlight, and should be compared with the present author's S (scotopic) curve.

1 *minus* the luminosity.[3] This latter condition appears not to have been reached within the range of the experiments considered here, except perhaps in the case of the process P_1; but no doubt it controls the threshold for brightness with full night-adaptation (as for instance in Piper's work).[4] Each individual band (curve) of activity should take a simple form, not unlike a

[3]For a treatment of this law, in the optical field, see Helmholtz, *Handbuch d. physiol. Optik* (2d ed.), 455. At the threshold, as Judd calls to my attention, there must be an allowance also for the idioretinal light.

[4]H. Piper, Über Dunkeladaptation, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 31, 1903, 161.

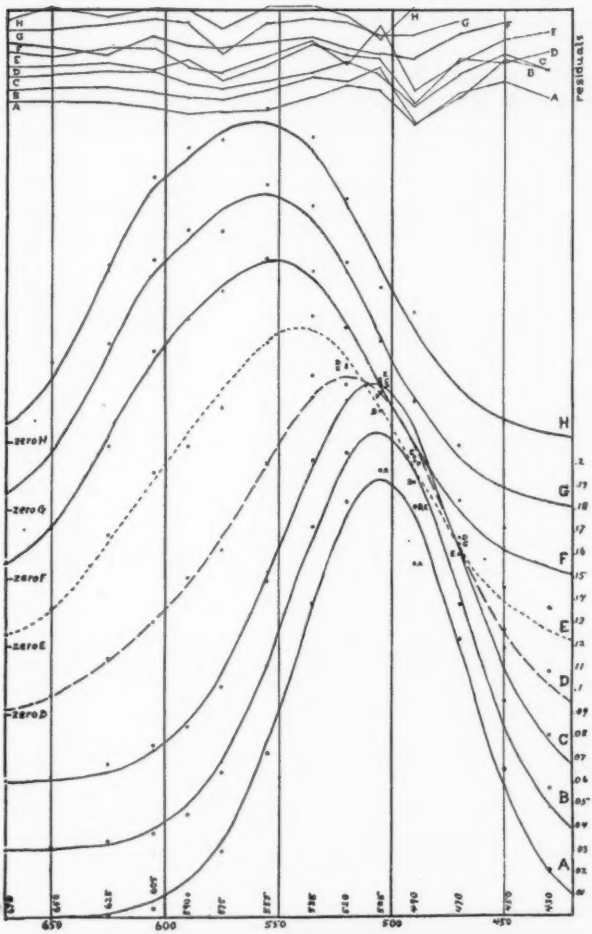

FIG. 2. KÖNIG'S 'HELLIGSKEITSWERTHE' FOR NORMAL *O* (CORRECTED), AND FITTED CURVES

The dots are the König values; the lines the fitted curves.

TABLE I

König's Coefficients

Reduced to sunlight and normal spectrum, using König's correction methods; also reduced to approximately unit sum

Normal Observer (König)	K. stage	H	G	F	E	D	C	B	A	S
	K. units	590	148	36.9	9.22	2.30	.575	.036	.00225	.00024
	Photons	295	74	18.5	4.61	1.15	.288	.018	.0011	.00012
	670 (mμ)	.0089	.0085	.0075	.0058	.0025	.00076	.00037	.00005	.000035
	650	.0348	.0268	.0221	.0160	.0084	.0026	.0011	.00017	.00008
	625	.0779	.0731	.0581	.0490	.0249	.0081	.0037	.0011	.0008
	605	.1166	.1101	.0999	.0766	.0408	.0163	.0076	.0038	.0026
	590	.1294	.1231	.1142	.0883	.0603	.0243	.0159	.0103	.0075
	575	.1330	.1227	.1264	.1048	.0723	.0419	.0343	.0293	.0202
	555	.1468	.1431	.1402	.1334	.1100	.0883	.0808	.0728	.0612
	535	.1340	.1334	.1349	.1451	.1490	.1419	.1372	.1388	.1328
	520	.1066	.1088	.1101	.1222	.1534	.1751	.1756	.1834	.1780
	505	.0674	.0738	.0813	.1171	.1451	.1759	.1935	.1974	.1935
	490	.0565	.0492	.0511	.0609	.1017	.1428	.1507	.1560	.1902
	470	—	.0281	.0336	.0400	.0773	.1044	.1080	.1222	.1267
	450	—	—	.0213	.0252	.0364	.0583	.0650	.0643	.0688
	430	—	—	—	.0162	.0182	.0200	.0267	.0203	.0170
Protanope (Ritter)	K. stage	H	G	F	E	D	C	B	A	S
	K. units	878	220	54.9	13.7	3.43	.857	.0536	.00335	.00037
	Photons	439	110	27.5	6.8	1.71	.428	.0268	.00168	.00018
	670 (mμ)		.0007		.00048	.00037	.00014	.00006	.00003	.00004
	650		.0029		.0020	.0015	.00082	.00019	.00012	.00012
	625		.0140		.0110	.0077	0031	.0011	.0010	.00098
	605		.0400		.0323	.0222	.0102	.0046	.0040	.0022
	590		.0744		.0662	.0485	.0221	.0116	.0090	.0068
	575		.1142		.1018	.0877	.0396	.0256	.0223	.0238
	555		.1534		.1498	.1410	.0832	.0664	.0604	.0677
	535		.1682		.1657	.1647	.1461	.1336	.1208	.1302
	520		.1483		.1551	.1636	.1897	.1766	.1661	.1852
	505		.1186		.1302	.1648	.1908	.2013	.2058	.1580
	490		.0860		.0880	.1046	.1536	.1723	.1750	.2025
	470		.0492		.0596	.0688	.1085	.1231	.1353	.1026
	450		.0223		.0283	.0355	.0383	.0592	.0781	.0952
	430		—		.0102	.0123	.0134	.0184	.0222	.0245

probability curve, and the absorption curve of the visual purple should either be of a similar form, or show wider bands embracing the bands of activity.

The obvious first assumption is that the pure scotopic curve, as shown for instance by the threshold brightness for the various wave-lengths, should be represented by the most sensitive substance alone. As a first approach to a solution, König's curves[5] (Fig. 2, plotted points) corrected for the prismatic spectrum and for the color of the source[6] were taken (see Table I) and from each

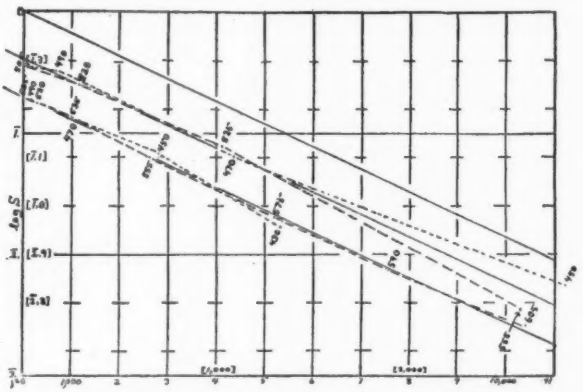

FIG. 3. FITTING OF GAUSSIAN TO KÖNIG'S S CURVE

The logarithm of the *Helligkeitswerthe* is plotted to the square of $\lambda - c$, c being taken as 503 mμ. The bracketed scale-values apply to the middle curve, and the upper line represents the Gaussian with maximal ordinate = 1.

$$[S = e^{-k(\lambda - c)^2}]$$

[5]König, *loc. cit.*, 168.

[6]König, *loc. cit.*, 225. I have reduced König's data to normal spectrum and sunlight by multiplying by the factors he himself gives. These are based on his own measurements of his own prism and a direct comparison with sunlight brought into his apparatus. They are probably not strictly equivalent to the modern 'standard noon sun'; but the divergence is probably within the range of uncertainty of the whole calculation. If the temperature was not exactly that for which the comparison with the sun was made, the uncertainty introduced may be larger, and this may explain in part at least the course of the blue end of König's curves, as a small difference in temperature causes a relatively large change in the amount of blue light. The yellow spot has not been allowed for in any of these data. It probably causes the steepness of the curve at about 520 mμ and the lack of fit in that region. It will also give the scotopic and blue coefficients a value lower than will truly represent the process in the retina itself, but a value which fairly represents their effective contribution to vision.

was subtracted a trial value of the probability curve which most closely fitted his threshold curve, S (Curve S of Fig. 1, also plotted as A of Fig. 2, and on a logarithmic scale as Fig. 3), so as to bring their blue ends approximately into coincidence. The resulting family of curves is shown in Fig. 4. It immediately became evident that the remainders (except for the blue end) could be ap-

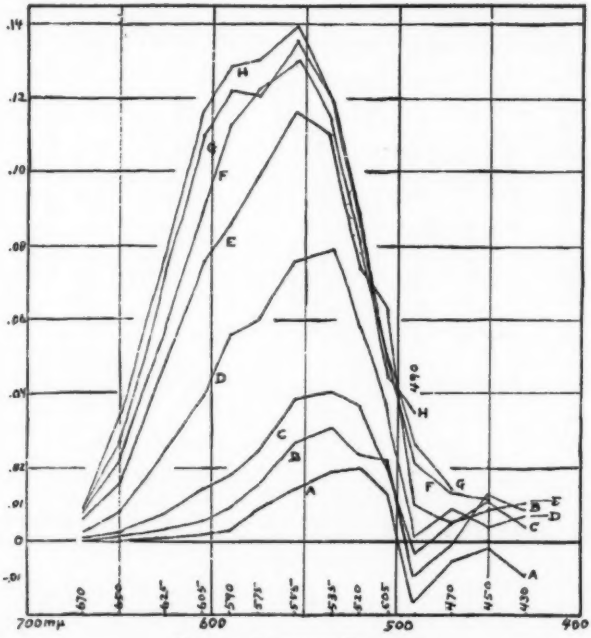

FIG. 4. PHOTOPIC RESIDUES
Obtained by subtracting trial scotopic values from König's values

proximately represented as the sum of two curves, one with a maximum at or near 550 mμ, which explained almost the whole residue at low brightness levels, and the other producing a well-marked secondary mode about 600 mμ, only obvious at the higher levels of adaptation. The well-marked change of *shape* of the curves, more prominent than the change of position of the maximum, shows that a postulate of shift of position of a simple process

cannot explain the case. Trial and error showed that the best fit is obtained with maxima at 605 and 548 mμ, and the definite depression between the two modes pointed to a very narrow band of activity.

For actual calculation of coefficients[7] curves were chosen somewhat wider than would make the best fit to König's data, because this depression was not shown by the mass of more recent

TABLE IIA

COEFFICIENTS OF THE ELEMENTARY PROCESSES

For approximately unit sum, at König's observational points

$P_1, P_2, S = Ce^{-k(\lambda - c)^2}$

λ	P_1	P_2	S
670 (mμ)	.0234	.0004	0
650	.0801	.0024	0
625	.1990	.0162	.0004
605	.2490	.0505	.0024
590	.2195	.0950	.0081
575	.1505	.1474	.0222
555	.0614	.1969	.0637
535	.0160	.1871	.1301
520	.0044	.1440	.1777
505	.0009	.0916	.2006
490	.00015	.0481	.1871
470	0	.0151	.1279
450	0	.0034	.0609
430	0	.0005	.0209
c	605 mμ	548 mμ	503 mμ
k	.00056	.000425	.000425
multiplier (C)	.249	.201	.201

work,[8] and it was desired to find a formula which should represent recent work as well as the König data. The final fitting was done by the method of least squares, as used by Judd[9] in fitting the three Young-Helmholtz excitations to the O.S.A. curve of brightness. This method is tedious for a single curve, but more manageable for such a family of curves as we have here, as only a few of

[7] As Hugo Schroeder (*Zsch. f. Sinnesphysiol.*, 57, 1926, 201) has calculated, the photon is about 2.1 König units. I have chosen the even number 2, which is within the probable uncertainty of the calculation.

[8] In Sloan's work even a narrower curve is needed. I have used the same curve for her parafoveal data as for König's, as the divergences were not so wide as to disguise the nature of the shift; but for her foveal data it was necessary to fit a narrower curve, and a value k was chosen for P_2 and P_3 of 0.000455.

[9] D. B. Judd, *J. Opt. Soc. Amer.*, 10, 1925, 635.

the coefficients in the resulting sets of probability equations have to be calculated for each curve. The coefficients of the three probability curves (Fig. 1) and their luminosities for the various wave-lengths are given in Tables IIA and IIB.

TABLE IIB

COEFFICIENTS OF THE ELEMENTARY PROCESSES
For unit sum at 10 mμ intervals from 400–700 mμ

λ	P_1	P_2	S
690 (mμ)	.0023	0	0
680	.0057	0	0
670	.0125	.0002	0
660	.0245	.0006	0
650	.0429	.0014	0
640	.0671	.0032	0
630	.0939	.0066	.0001
620	.1178	.0129	.0004
610	.1316	.0227	.0009
600	.1316	.0369	.0021
590	.1178	.0550	.0047
580	.0939	.0751	.0093
570	.0671	.0946	.0173
560	.0429	.1092	.0292
550	.0245	.1159	.0455
540	.0125	.1131	.0649
530	.0057	.1013	.0850
520	.0023	.0831	.1029
510	.0008	.0629	.1138
500	.0004	.0437	.1156
490	.0001	.0278	.1082
480	0	.0163	.0926
470	0	.0088	.0731
460	0	.0043	.0530
450	0	.0020	.0352
440	0	.0008	.0215
430	0	.0003	.0121
420	0	.0001	.0058
410	0	0	.0029
400	0	0	.0015
c	605 mμ	548 mμ	503 mμ
k	.00056	.000425	.000425
multiplier	.1336	.1162	.1162

The calculated percentages of the three processes at König's experimental brightness levels are given also in Table III.

If we plot the resulting curves (Fig. 2) with the corresponding König data (plotted points), we find that there is a fair fit, but

that the marked separation of the modes at 605 and 548 mμ is partly smoothed. Still we may say that the curves fairly represent the König data. On the other hand, it will be seen that in the case of all the recent data except Sloan's, none of which was taken over a range of luminosities sufficient to give any independent check of the law of variation with brightness, there is about an

TABLE III

PROPORTIONS OF THE ELEMENTARY PROCESSES
Fitted to König's data by the method of least squares

	Brightness	Proportions			Proportions ÷ Brightness		
König	Photons (approx.)	P_1	P_2	S	P_1	P_2	S
S	.00012	0	0	1.	0	0	2000.
A	.0011	0	.092	.92	0	20.4	204.
B	.018	0	.135	.855	0	1.875	11.8
C	.288	.006	.201	.78	.005	.175	.678
D	1.15	.088	.362	.538	.019	.0805	.117
E	4.61	.185	.535	.272	.010	.0291	.0148
F	18.5	.27	.57	.16	.00365	.0077	.00243
G	74.	.30	.575	.1115	.0011	.00195	.000378
H	295.	.33	.57	.115	.00028	.00048	.0001
		(Protanope)					
S	.00018	—	0	1.			
A	.00168	—	0	1.			
B	.0268	—	.04	.96			
C	.428	—	.175	.85			
D	1.71	—	.555	.455			
E	6.8	—	.64	.36			
F-H	27 - 440	—	.79	.16			
		(Deuteranope)*					
H	336	.26	.67	.06			

*Rough calculation, rejecting the point at 520 mμ.

equally good fit, but the errors are of the opposite sign, the separation between the modes being much too deep—for these modern curves show hardly a trace of the depression about 575 mμ.

To test the inverse relation between brightness of stimulus and concentration of pigment, we may divide each coefficient by the absolute total brightness of its level, and plot the resulting figures (Table III, columns 5–7) on a logarithmic scale. In this case the curve of total brightness plotted against total activity is of course a straight line, and the curves of the individual components should approximate to straight lines (Fig. 5). It is obvious that this relation is approximately true, but that some

disturbing factor comes in at about 4 photons. The simplest explanation of conditions appears to be that adaptation is not wholly independent in the three processes, but that they interfere to some extent with each other; also the basic assumption on which the calculation is made is not wholly valid, this assumption being that equilibrium brightness (nerve activity) is a constant at all levels of adaptation. This is far from strictly true; but the change in brightness is not measurable in any way we have yet discovered, and it is certain that a total change in luminosity of the stimulus of a hundred thousand- or a million-fold corresponds to only a very moderate change in subjective brightness.

A final adjustment is theoretically possible. We may draw smooth curves approximately through the points of Fig. 5 and Fig. 6, and read the excitations from these curves. The resulting excitations, adjusted to add up 1, are shown in Table IV.

TABLE IV

PROPORTIONS OF THE ELEMENTARY PROCESSES
(Smoothed)

Photons	P_1	P_2	S	Photons	P_1	P_2	S
0.0001	0	0.035	0.965	1.	0.04	0.32	0.64
0.001	0	0.05	0.95	10.	0.22	0.54	0.24
0.01	0.001	0.09	0.91	100.	0.30	0.575	0.125
0.1	0.01	0.15	0.84	1000.	0.34	0.55	0.11

DISCUSSION

The scotopic factor. The scotopic factor can be taken as sufficiently established. König[10] has already noticed the close parallelism of the curves of the threshold of vision, the sensitivity curve of a completely color-blind *O*, who had only the scotopic process present, and the absorption curve for visual purple, prepared under dim orange or red light, and therefore composed mainly if not wholly of the scotopic substance.[11] Köttgen and Abelsdorff[12] later examined many other animals and found essen-

[10]König, *loc. cit.*, 346 f.

[11]The scotopic substance must be many times more concentrated than any possible photopic substances, to explain the ratios of sensibilities of the two processes at their threshold. Under the conditions of preparation of the visual pigment (with red or orange light) the scotopic pigment was presumably all in its active (visible) state, while the photopic, being more or less sensitive to red light, was undoubtedly largely bleached.

[12]E. Köttgen and G. Abelsdorff, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 12, 1896, 161.

tially identical curves for all above the fishes. The probability curve which I have adopted to represent S (Table II, column S) lies in the midst of these curves, and may be taken as a fair representative of them all.

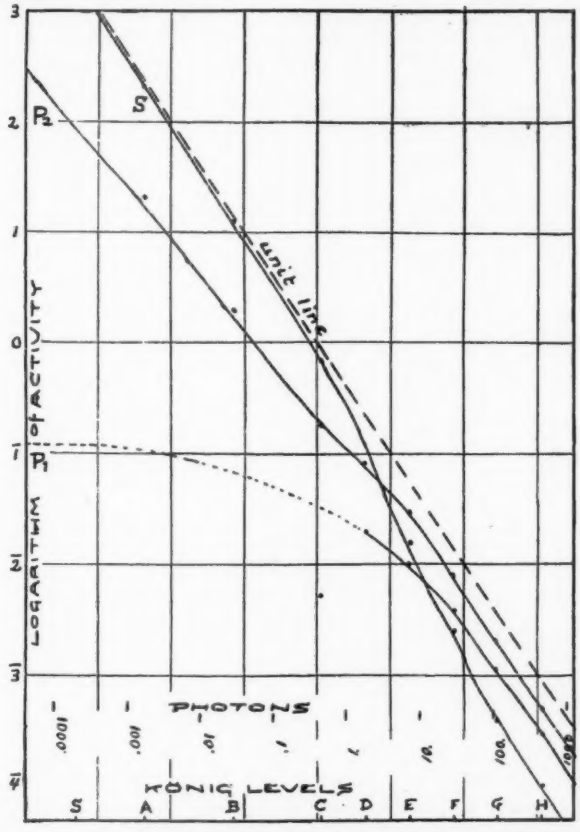

FIG. 5. ACTIVITY OF RETINA (COEFFICIENTS OF P_1, P_2, S, DIVIDED BY BRIGHTNESS) PLOTTED AGAINST BRIGHTNESS ON A LOGARITHMIC SCALE, FOR EXTRAPOLATION

The first photopic factor (P_1). The first photopic factor (P_1) is perhaps the least well established as to position and extent

(though its existence and approximate character are required by the data), as it does not appear in any approach to isolation either in the sensitivity curves, even at the highest brightnesses,[13] in any color-blind cases, or in the absorption spectra of visual pigment. Nevertheless, a glance at the series of curves in Fig. 2 will show that some such process is necessary to explain the manner in which the left side of the curve gradually rises with

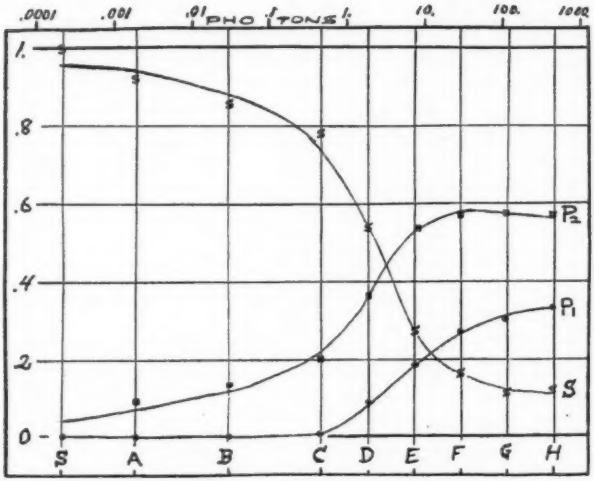

FIG. 6. EXCITATIONS PLOTTED FOR INTERPOLATION

increasing brightness. It is interesting to compare this process with the 'red' process of the Young-Helmholtz theory. The two are closely similar in position, but there seems to be a definite divergence. The process, as I identify it, has a maximum at about 605 mμ,[14] and with an illumination of about 50 photons contributes about 25% of the brightness sensation; the formulation usual on the Young-Helmholtz basis uses a maximum at below 590 mμ, and covers a substantially wider range of wave-

[13]L. L. Sloan (*Psychol. Monog.*, 38, 1928, no. 173) found that the red process was relatively very highly sensitive to dim light in the photopic eye, as shown by the next to bottom row of graphs on her Fig. 8, facing p. 68. As equilibrium approached the red process dropped to its usual position of subordination.

[14]Even higher in Sloan's foveal runs.

lengths. Judd's calculation on this basis would indicate that the process contributes 45% of the brightness. This divergence is well beyond the probable range of uncertainty of my curve.

The second photopic factor (P_2). The position of the maximum of the second photopic factor (P_2) is fixed by the convergence of data from color-blind (protanopic) cases as well as the analysis given above. In the protanope, the first process is evidently negligibly small or absent, and the long-wave side of the second process is laid bare. In fact the protanopic case which König analyzed with some completeness can be matched with fair accuracy by a system of two components (Fig. 7). It is interesting to note that the absorption curve of *fish* visual pigment (Fig. 9) as given by Köttgen and Abelsdorff, shows a maximum at about the same point, and that attempts to bleach it differentially with colored light failed to show any indication of heterogeneity; so it seems as if the fish had a single pigment corresponding to this process. The main difficulty is that the absorption band of the fish visual pigment is much wider than would fit König's data (Fig. 9). König found one observer[15] who had a curve in the red-green region fairly representing this process alone, and who was night-blind, therefore presumably lacking the scotopic substance; but the blue region was not investigated in his case.

This curve corresponds closely to the 'green' curve under the Helmholtz theory. A fair approximation is necessitated by the data; but the exactness of fit is due to the fact that the present curve was derived on the assumption that the protanope has only two processes contributing to the sensation of brightness. In fact the residuals in the normal *O* (Fig. 2, top) show a systematic drift which indicates that this second photopic curve should be somewhat narrower and farther toward the short waves, and that the protanope measured by König should be allowed a small proportion of the first photopic process.

A third photopic factor (P_3). The approximate coincidence of the two preceding processes with the red and green processes demanded by the Young-Helmholtz theory, suggests a search for a third, a blue, factor. In fact a study of the residuals (Fig. 2, top) seems to show a hump centering at about 450 mμ, the presumable position of such a third process. The light used by

[15]König, *loc. cit.*, 332.

König was weak in the blue, and it was not possible for him to carry his brighter runs to the blue end; with the dimmer lights the process is certainly weak in proportion to the scotopic process which overlies it, and the observational values are so ragged that

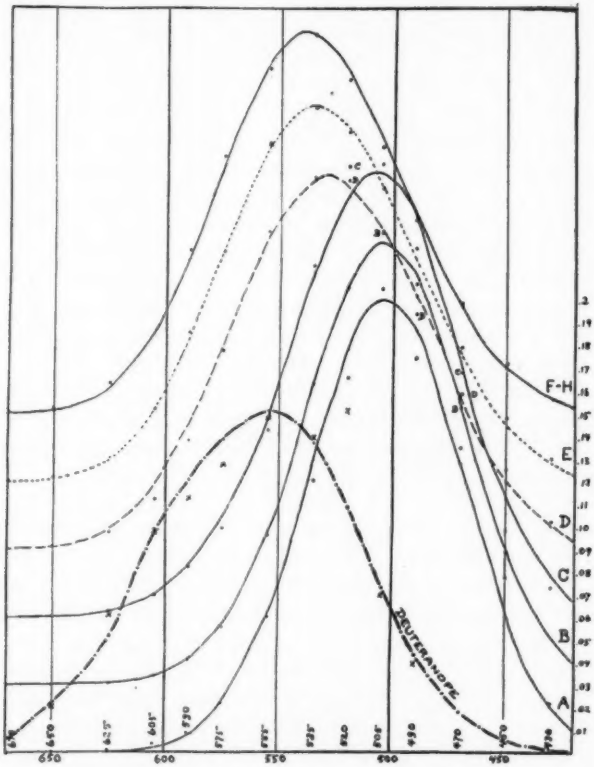

FIG. 7. KÖNIG'S HELLIGKEITSWERTHE FOR COLOR-BLIND *Os*: PROTANOPE (RITTER) AND DEUTERANOPE (BRODHUN), WITH FITTED CURVES

no definite conclusion is possible. The only modern data over any considerable range of luminosities (Sloan's) likewise show but little evidence of this process, though in other recent data, where the scotopic factor is eliminated by using a stimulus area more nearly limited to the fovea, a small mode is visible, but even

smaller than König's curves would lead one to expect. If the process is present it would seem to have a mode at about 450 mμ and a coefficient (P_3) between 0.05 and 0.1 from König's data, or a coefficient of about 0.02 according to more recent figures. The figure shows no definite drift with changing adaptation, unlike the other factors, and may be an artifact, caused by some moderate error in the factors used in reducing the observational values (based on artificial light) to sunlight. If the observational light were a little whiter than supposed, it would produce some such distortion of the curve.

Color-blindness: (*a*) *Protanopic.* König has studied a single protanopic *O* (Ritter) over the whole range of brightness that he used. His data, when corrected for the prism and the color of the source used (see Table I), can be matched with the use of only two of the three processes necessary for a normal *O*, even more closely then the three processes represent the normal *O*. The coefficients do not run so smoothly as in the normal case, the Purkinje shift taking place mainly between König's levels C and D, *i.e.* about 0.4 and 1.75 photons (his units being apparently about 0.5 photons), and there being no further shift reported by König above about 25 photons, where in the normal observer the shift is largely due to the change in the proportion of the two photopic processes. (See Fig. 7.)

The calculated coefficients are as follows.

Brightness		Proportions		
König designation	Photons (approx.)	P_1	P_2	S
A	0.002	0	0.00	1.00
B	0.027	0	0.04	0.96
C	0.43	0	0.175	0.85
D	1.72	0	0.555	0.455
E	6.90	0	0.64	0.36
F-H	28. – 425.	0	0.79	0.16

As actually calculated, a few of the coefficients for P_1 have small negative values, showing that our P_2 is a little too wide, even on the assumption that P_1 is wholly absent in the protanopic case.

For the protanope, then, the effect is as if the red process had been eliminated from the equation of brightness, as well as from the color sensation. If this case stood alone, there would seem to

be very definite evidence that the color and brightness processes were identical. On the other hand, the deuteranope shows a very different situation.

(*b*) *Deuteranopic.* König gives one definite set of quantitative data on a deuteranope, and Gibson and Tyndall have measured a second *O*,[16] who from the incomplete data presented is either a deuteranope or an extreme deuteranomalous case. König examined his *O* at two levels only; the first level, A, showed as is to be expected only the scotopic process in its usual form, and is of no special significance. His brightest level (H), however, is very striking (Fig. 7, dot-and-dash curve). Instead of being a simple sum of two processes, as in the case of the protanope, the curve is essentially the normal curve, though with a slightly low value for S and high value for P_1. The second photopic process, P_2, which should be wholly absent if it represented merely the green process, is not only present, but produces the maximal point of the curve; while the process P_1 (red), which should be dominant, merely produces the usual well-marked hump on the long-wave side of P_2. In fact there is *no* simple curve which can be fitted to the photopic portion of this curve, while by assuming the same two processes present as in the normal *O* the fit is satisfactory. In this case, therefore, we have only *two* color processes on the Helmholtz theory, or one on the Hering theory, as the color blindness shows, while the brightness curve shows as plainly as possible the separate modes for 'red' and 'green.'[17] This seems to point strongly to a situation where there has been a de-differentiation of the color-process, without any disturbance of the brightness process. The simplest explanation on a Helmholtz basis would be that the red and green processes were present as usual, but had no separate connection with the brain—adding their respective brightnesses, but producing only a single color sensation. On the Hering basis it would indicate the presence of all the normal pigments in the

[16]K. S. Gibson and E. P. T. Tyndall, Visibility of radiant energy, *Sci. Pap. U. S. Bur. Stand.*, 19, 1923 (no. 475), 154. They are not clear as to whether this was a deuteranope or an extreme deuteranomalous observer. Judd informs me this *O* was not strictly deuteranopic, but the published data give no hint of ability to distinguish between red and green.

[17]It is obvious that, as we have only data at a single brightness level, it will be entirely possible to fit an *unsymmetrical* curve to the photopic portion of this curve, but the curve would not be even an ordinary skewed one, but would have to be bimodal. This is practically what the ordinary Helmholtzians have done in deriving their 'red' curve from the deuteranopic observer.

white-organs, but the absence of the red-green system of color-organs, while the protanope would have lost one of the pigments as well as the same set of color-organs. If that were the explanation the reverse defect might also occur, with the color-system normal, but with a brightness curve similar to that of König's *O* (Ritter). No such case appears among the relatively few for which I have seen quantitative data; they would be much more easily overlooked than a color-blind individual, but if at all common should have appeared among Crittenden and Richtmyer's or Gibson and Tyndall's cases.[18] The second possibly deuteranopic case is Gibson and Tyndall's *O* 23.[19] This *O* is not fully described, and I understand was not fully tested, but he is reported as making the usual color-blind confusions, and as his brightness sensation is not defective at the red end must have been a close approximation to a deuteranope. In his case also the coefficients are similar to those of a normal *O* at the same brightness levels, in fact the curve falls well within the experimenters' normal group. As the brightness was not held constant, and appears to lie in a region where the Purkinje shift is rapid, the actual figures of the three processes are of little significance. Still the shoulder on the curve at 600 mμ and the location of the principal mode are just as in König's case.

Modern Researches

When we come to the modern observations, some difficulties appear in relating them to König's data. With the exception of Sloan's very recent work, which will be separately discussed below, none of the modern work has been done over any considerable range of brightnesses. The greatest range is Ives' set, with a ratio of 100 — 1; and even this set lacks the blue end.

This group of experiments as a whole stands in contrast to König's in showing no real evidence of a scotopic component. In general the brightness has been higher than his, and the field seems to have been smaller than in König's case; but even allowing for these two factors, there seems to be some divergence on this point. There is also a curious disagreement between the methods of flicker photometry and of equality-of-brightness, commented on by

[18]E. C. Crittenden and F. K. Richtmyer, An 'average eye' for heterochromatic photometry and a comparison of a flicker and an equality-of-brightness photometer, *Sci. Pap. U. S. Bur. Stand.*, 14, 1918, (no. 299), 87-114; Gibson and Tyndall, *loc. cit.* Among this rather large number of subjects there are several with the slightly enhanced red-sensibility characteristic of the deuteranope, but none with the red-defect of the protanope.

[19]Gibson and Tyndall, *loc. cit.*; (cf. Note 16).

Ives. This is presumably due to the psychological invalidity of the flicker photometer, whose function breaks down completely at a level somewhat below that of Ives' observations; but the point calls for further study.[20]

This group of curves also calls as a whole for wider wave-length ranges for the component processes, the relative depression between the two photopic modes (at about 575 mμ) being obscure or even absent. There was considerable variety in comparison lights, but so far as I know none duplicated König's use of a monochromatic comparison light.[21]

Ives published two series of data,[22] the first obtained by both the flicker and equality of brightness methods over a range of brightnesses, and the second limited to flicker data at the brightest level, with a small field.

His data are defective at the blue end, and so cannot be used as evidence in the matter of a separate blue process. The earlier set, which used a dim light (about 0.25 meter-candles), lacks even the region where a scotopic process might be revealed; but the half-curve which they show (from the red to the green) is very interesting as indicating a reversed Purkinje shift with the flicker. The cause of this is obscure. It may be in part connected with the sensation of flicker, whose mechanism is unknown and which may involve a time factor and perhaps other factors as well as pure brightness. Also the stimuli used included (as usual) both foveal and parafoveal regions. In this case there seems to be some sort of rivalry between the two, and the judgments of equality may in Ives' case have been made with the fovea, which from the little evidence we have appears to lack the scotopic process, and may be unique in other ways.[23]

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[20]Sloan's observations on more purely foveal stimuli seem to suggest that the behavior with the flicker photometer is that produced by a pure foveal stimulus, while that with the equality of brightness method more nearly represents that due to the parafoveal region. It looks like a case of rivalry between the two zones of the retina. See the last paragraphs of this paper.

There appear to be at least two parafoveal zones differing in scotopic sensibility, besides the fovea. The inner one becomes blind at a level much lower than that at which the fovea becomes night-blind, perhaps a quarter as bright, but at a higher level than the zone of five or ten degrees diameter which may well have been included in König's field.

[21]König used a yellow-green spectral comparison light (535 mμ), which undoubtedly fatigued the retina differentially for component P_2, as well as the light being measured, and probably reduced the coefficients of P_2 to below their true value. Ives used a white (or rather yellow) comparison light and probably got the closest approach to an eye not fatigued for color; Gibson and Tyndall apparently had no comparison light or surrounding field, and the light being measured undoubtedly fatigued their eyes very unequally for the several component processes. It is not clear what effect this would have on their measurements; but these three sets of data are evidently on the whole far from comparable. The ideal device would be to use a white background of fixed brightness to keep the eye in a steady state of adaptation, cutting off this light as briefly as possible while observations were being made. Sloan did this; but her exposure periods (2 sec.) were perhaps too long in the case of the dark-adapted eye, and she did not use adaptation levels near those of the stimulus, but only one brighter and one apparently much dimmer than her whole series of stimuli.

[22]H. E. Ives, *Phil. Mag.*, 24, 1912, 149-188, 845-863.

[23]See the discussion of Sloan's foveal data at the end of this paper.

Ives' second run was at a single brightness. Calculation of the coefficients, on the assumption that the curves extracted from König's data are correct, gives the two upper curves of Fig. 10. There is a fair fit, but the calculated values are too low at 590 mμ and too high at 550, indicating that the curves of the elementary processes are too narrow. Ives' mean curve fits fairly well on the whole, but his personal curve is too steep on the blue side, the effect to be expected from abnormally heavy yellow pigment. The coefficients work out

	P_1	P_2	S
Mean curve	0.22	0.78	0.004
No. 17 (HEI)	0.19	0.81	0.003

The S values are insignificant, of course.

Gibson and Tyndall[24] used the method of 'equality-of-brightness' by small steps. As they used this method it gives the rate of change of brightness (dy/y) at a series of selected points along the visibility curve. The coefficients as published by them are only in part at the points where observations are stated to have been taken, and no statement is made as to how they were interpolated. The method also requires a calculation along from point to point, and some assumption (I am not sure what) of the course of the visibility curve between observed points. Their brightnesses were not held constant and fluctuated over a range of 10 to 1 between the limits of 690 and 450 mμ, and much more at the extreme ends. If we allow that adaptation caused about as much shift as is indicated by König's curves E, F, and G, which seem to cover this range of brightnesses, we can apply corrections. A rough calculation shows that this would not change the general shape of Gibson and Tyndall's curves, but would visibly improve the fit at the red end where their curve fairly matches König's curve E, and much more strikingly at the blue end, where at least half of the irregularity on which they comment seems to be a direct reflection of the irregular changes in brightness. There seems to be a residue, which from its form would suggest a blue process (P_3) of about 2%. As in the other cases, the scotopic component comes out negligibly small. The coefficients work out

	P_1	P_2	S
Average	0.23	0.76	0.01
(EPTT)	0.30	0.70	negative[25]

One of Gibson and Tyndall's *O*s was a deuteranope or substantially red-green weak. It is striking that his curve, while red-strong, falls within the range of Gibson and Tyndall's normal *O*s, showing no substantial defect in the second ('green') photopic process; in fact the curve is a close match to Tyndall's own. The coefficients work out (assuming S as zero) $P_1 = 0.306$, $P_2 = 0.686$, P_3 = about 0.03. P_3 should be reduced on account of the variation of brightness to 0.02 or less and may be illusory.

On the whole, Gibson and Tyndall's curves fit the coefficients rather surprisingly well, except for the extra steepness from 500 to 520 mμ where the yellow pigment causes its usual distortion.

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[24]Gibson and Tyndall; (cf. Note 11).

[25]Note that Tyndall is as red-sensitive as a deuteranope.

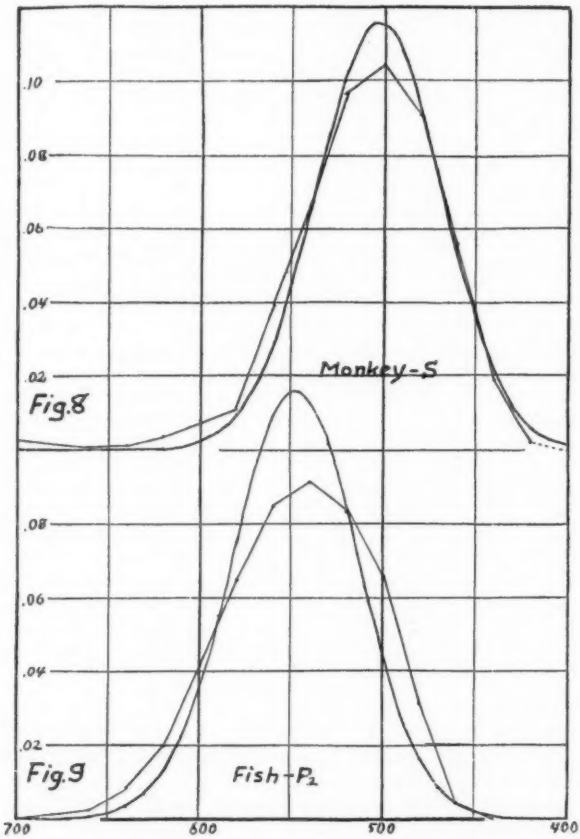

FIG. 8. CURVE OF ELEMENTARY PROCESS S SUPERPOSED ON ABSORPTION OF VISUAL PURPLE
(monkey)

FIG. 9. CURVE OF ELEMENTARY PROCESS P_2 SUPERPOSED ON ABSORPTION OF VISUAL PURPLE
(fish)

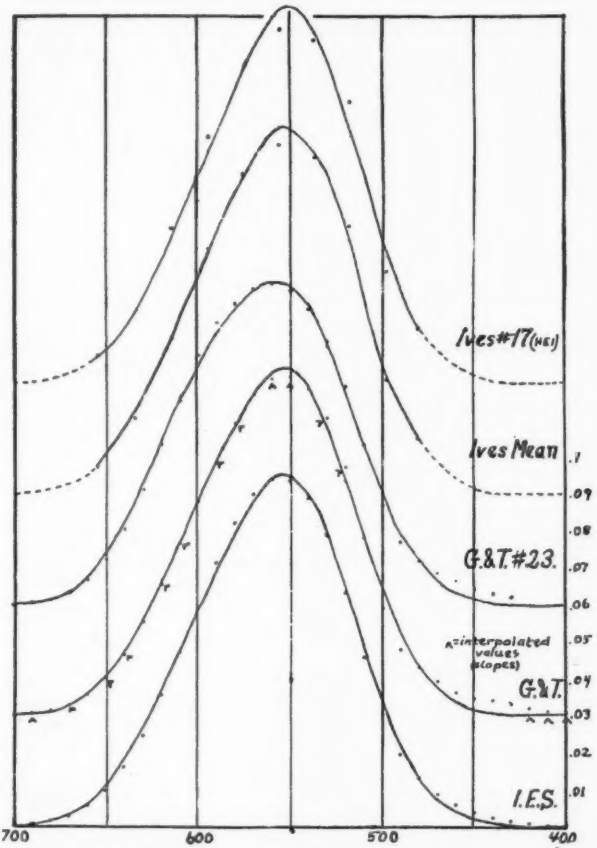

FIG. 10. VARIOUS MODERN DATA ON EXCITATIONS, WITH FITTED CURVES

The Illuminating Engineering Society's curve was a compilation,[26] but I calculate it because it was used as a basis for Judd's calculation.[27] The fit is not quite so good as with Gibson and Tyndall's curve, or for that matter quite so good as Judd's solution. It is too high at 580 mμ, pointing to wider components, as in the other modern curves, and there is the usual small (blue) excess, of less than 2%. The coefficients are $P_1 = 0.23$, $P_2 = 0.77$, $P_3 = 0.015$, and $S = 0$. P_3 is not used in the plotted curve, but estimated from the residuals.

[26] *J. Opt. Soc. Amer.*, 4, 1920, 58; 6, 1922, 541.

[27] D. B. Judd, *ibid.*, 10, 1925, 635.

Visual pigments. Köttgen and Abelsdorff give a considerable range of data on the extracted visual pigments of the eyes of various vertebrates.[28] If the visual process is primarily a photochemical one in which the visual pigment functions, the pigment which is present in large quantity should be that which serves in the scotopic process. As already noted by various workers (*e.g.* König) the absorption of the visual purple corresponds closely to the scotopic visual process. On plotting Köttgen and Abelsdorff's curves the fit seems very fair, with an allowance of some 3 mμ for the index of refraction of the cones (Fig. 8), but there appear some irregularities at the red and blue ends. The latter (not obvious in the specimen figured) are presumably caused by a slightly incorrect allowance for the yellow tint of the blood mixed with the extracted pigment and the color of the solvent. The red residuals are suggestive in the present connection. In the extraction of the pigment, red or orange light was used. In the latter case there seems to be a definite excess at the red end, possibly indicating that a little of the P_1 pigment had escaped bleaching by the orange light used, while in the case where red light was used the excess is more widely distributed and may well be caused by the survival of some P_2 as well as P_1. The amount of this excess varies a great deal in different determinations by Köttgen and Abelsdorff, as might be expected to result from varying exposures to the red light during the preparation.

Another interesting comparison is of the different pigment found by Köttgen and Abelsdorff (Fig. 9) to be general in the fishes. This matches fairly well with the curve for P_2, and suggests that it is really the pigment of P_3. Attempts to bleach this pigment differentially with colored lights failed, and the fishes presumably have only a single pigment; but there seemed some hint of success in higher animals. Possibly by operating under blue light free from red (the common blue glass transmits some red light) there might be greater success in uncovering the photopic pigments.

After the main part of this paper was finished, Sloan's study of the visual function for various states of adaptation and various stimuli was brought to my attention.[29] So far as I know this is the only paper in the field so far that has attempted to fix both physical and physiological conditions, so that the condition of the eye as well as the stimulus may fairly be said to be known. Only two adaptation levels were used for practically all of the work, one brighter (135 mc. = 260 photons) and the other dimmer than the series of stimuli used. In the case of the bright-adapted eye, the method probably really kept the adaptation level practically constant, as the eye when in a photopic condition adapts very slowly to *decreasing* illumination, and stimulus-times were short. On the other hand, the eye adapts very rapidly under scotopic conditions, and much more rapidly up than down, so that it is not unlikely that in the experiments with the dark-adapted eye, the adaptation was more nearly to the stimulus light than to the (unstated) surrounding field. The

[28] Köttgen and Abelsdorff, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 12, 1896, 161.

[29] L. L. Sloan, The effect of intensity of light, state of adaptation of the eye, and size of photometric field on the visibility curve, *Psychol. Monog.*, 38, 1928, (no. 173).

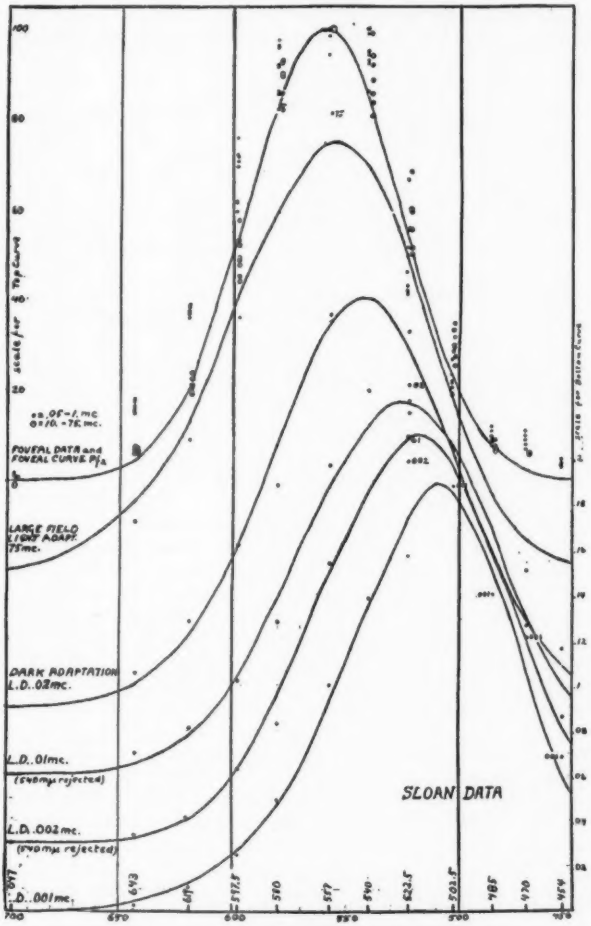

FIG. 11. SLOAN'S DATA WITH FITTED CURVES

The four lower graphs are for the dark-adapted eye with weak stimuli and large (5°) field. The next is of the brightest stimulus used, light-adapted eye and the bright field. The upper curve is the 'green' elementary process, moved 10 mμ toward the long waves, superposed on eight sets of Sloan's values for the fovea (57′ field). The circles are values for bright stimuli and the solid dots for faint stimuli.

lower values as given by Sloan, then, are nearly comparable with König's experiments, and should show a similar course of adaptation in the case of the parafoveal field, as in fact they do (Table V, Fig. 11). The method of equality

TABLE V

SLOAN'S DATA

Adaptation	Illumination mc.	factor	photons	P_1	P_2	S
dark	.001	3.93	.0039	.025	.205	.757
"	.002	3.875	.0078	—	.38	.63 ‡
"	.01	3.66	.0365	.024	.455	.48 ‡
"	.02	3.54	.0708	.048	.726	.198
"	.2	3.09	.618	.186	.790	.023
"	5.	2.39	11.95*	.252	.778	negative
"	10.	2.275	22.75*	.285	.763	"
"	20.	2.16	43.2 *	.221	.820	"
"	40.	2.06	82.4 *	.259	.788	"
light	75.	1.91	143 †	.209	.811	"

*These values are calculated on the basis of adaptation of the pupil to the stimulus light; they should be somewhat increased on account of the exposure of the eye to a dark field between exposures to the stimuli. The factor for a completely dark-adapted eye is about 4.

†Based on the brightness of the surrounding field, 135 mc. equals 258 photons.

‡Omitting the irregular value at 540 mμ.

of brightness was used, and it is interesting to see that the results are more nearly comparable with König's than any other recent work, though on the whole somewhat more irregular. The scotopic values for corresponding illuminations are lower than in König's case, correlated with the smaller field.

Sloan's work is the first quantitative work I know of which gives data on the fovea fairly free from the influence of parafoveal regions of the eye.[30] She used a stimulus area of 57′ diameter, which probably included only a very small amount of parafoveal retina.[31] Her results show practically no change of maximal sensitivity with adaptation, and only a very small change in the shape of the curve. Her curves are so nearly identical that I have averaged two groups of eight each to study the elementary processes, in the hope of reducing the accidental error, which seems rather large in the individual curves.

It is definitely impossible to get a good fit with the elementary curves used for the parafoveal region, but if the maximum of each curve is moved about

[30]Sloan used the natural pupil. In calculating the retinal illumination for comparison with other work, I assume that with the bright adaptation-field the eye was adjusted to the adaptation-field, but that without the adaptation field the iris was in a state corresponding to the stimulus light, or for the brighter stimuli at a somewhat lower level. This is arbitrary but seems the closest approximation possible in the circumstances. I have interpolated the correction factors for size of iris from Schroeder (*Zsch. f. Sinnesphysiol.*, 57, 1926, 201 ff., esp. 213), which is also a source of information for a variety of optical units.

[31]The fovea is a horizontal ellipse of somewhat irregular outline, and its minimal height, projected, is probably less than 57′.

10 mμ to the lower wave-lengths a good fit is possible.[32] It is also necessary to decrease the width of the 'green' curve, at least. The following is a rough estimate of the resulting coefficients;

	Pf_1	Pf_2	Pf_3	Sf
Wave-length maximum (c).	617 mμ	560	462	515
k	0.00056	0.000455	0.000455	0.000425
Sum of 8 curves, 0.05–1. mc.	0.14	0.81	0.05	0
Sum of 8 curves, 10–75 mc.	0	0.94	0	0.06

This result seems wholly anomalous, but is much like Ives' results with the flicker photometer. It seems to indicate that whatever is the mechanism by which the eye judges brightness, in any case the fovea uses almost wholly the 'green' process. While the fovea has long been suspected of blue-weakness, the negligibly small red value with day-adaptation is entirely unexpected. Ives used a substantially larger field, probably containing a larger area of parafoveal than of foveal retina. His work would be brought in line with this, if we can assume that in judging with the flicker photometer the true fovea is dominant, while with the method of equality of brightness the parafoveal region dominates in proportion to its larger area.

At the top of Fig. 11 I have plotted the curve of Pf_2 as estimated here, and the corresponding group of Sloan's observed values. It shows both the very good fit about the maximum and the divergence at the ends, indicating the need for some small activity of the other processes to explain the observations.

Summary

The relative sensitivity of the eye to light of different wave-lengths varies with the brightness of the stimulus and especially with the state of adaptation of the eye (Purkinje phenomenon). If we assume that the sensitivity of the eye is primarily due to the presence of photosensitive pigments (visual purple and corresponding photopic substances) the curve of sensitivity of the eye should correspond to the sum of the curves of activity of these substances. It is assumed (*a*) that the band of activity of each substance is of simple form, to be represented by a probability curve (activity $= e^{-k(\lambda-c)^2}$;) (*b*) that the band for each substance is unchanged with state of adaptation of the eye, but that the relative proportion of the active pigments is changed;

It is found that the whole range of change of sensitivity of the eye with changing adaptation can be represented as the

[32] The position of the absorption band of a colored substance depends on the substance in which it is dissolved. This amount of shift in wave-length seems large, however, to be explained merely on the basis of a difference of constitution between the foveal organs and ordinary cones.

sum of three such processes (with doubtful indication of a fourth). Two or three are photopic (called P_1, P_2 and perhaps P_3) and one scotopic (S). The coefficients of the corresponding probability equations are

	c	k
P_1	605mμ	0.00056
P_2	548	0.00042
P_3	450?	0.00042?
S	503	0.00042

The values of c (the center of the absorption curve) are fairly definitely fixed by the data, those of k, representing the width of the bands of activity, are more uncertain, and König's and Sloan's data seem to indicate much narrower bands of activity than other recent data (none of which is based on any substantial range of brightness).

For König's data (outer parafoveal region) the coefficients of the three processes for full day-adaptation are: $P_1 = 0.33$, $P_2 = 0.57$, P_3 = not over 0.04, S = 0.11. For Sloan's data, representing a smaller inner parafoveal area, $P_1 = 0.2$, and $P_2 = 0.8$, with no definite evidence of P_3 or S. At full dark adaptation all observers get the S curve alone. For laboratory intensities (say 20 to 50 photons) König with a large field gets up to 25% of the scotopic process, modern observers with smaller fields none above about 5 photons.

A protanope observer (König data) lacks P_1; a deuteranope has both P_1 and P_2 of nearly normal value.

The fovea cannot be brought into this scheme, but its sensitivity can be almost matched at high brightnesses by the curve P_2 alone, moved about 10 mμ toward the longer waves. P_1 appears at low brightnesses, but S is negligibly small at all brightnesses at which the fovea is sensitive. (It may be absent, as no data have been published on absolutely pure foveal stimuli.) Ives' results with the flicker photometer correspond to Sloan's with the fovea, while his results by equality of brightness represent the parafoveal phenomena as reported by other observers.

WHOLE AND PART METHODS WITH NON-SERIAL REACTIONS

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The purpose of this experiment is to compare the efficiency of the whole method and of various types of the part methods for the learning of those non-serial reactions which are involved in card sorting. Previous experiments in this field have dealt almost exclusively with serial reactions, *i.e.* those which must be practiced and acquired in a fixed and unvarying order, and which therefore constitute a definite chain or series in which each response becomes an essential stimulus for the next. The numerous investigations in the learning of poetry,[1] prose,[2] lists of nonsense syllables,[3] piano pieces,[4] mazes,[5] mirror drawing,[6] are cases in point.

It is not our intent here to discuss or evaluate the results of these numerous experiments dealing with serial reactions. The facts are in any case diverse, possibly even contradictory, and suggest that the relative economy of whole and part methods may well be a variable influenced by many factors, such as kind and difficulty of material, amount of practice, age, capacity and learning habits of the subjects. Furthermore, whatever be the

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[1] *E.g.* M. E. Lakenan, The whole and part methods of memorizing poetry and prose, *J. Educ. Psychol.*, 4, 1913, 189-198; G. Pentschew, Untersuchungen zur Oekonomie und Technik des Lernens, *Arch. f.d. ges. Psychol.*, 1, 1903, 409-526; E. W. Sawdon, Should children learn poems in wholes or in parts? *Forum Educ.*, 5, 1927, 182-197; Lottie Steffens, Experimentelle Beiträge zur Lehre vom ökonomischen Lernen, *Zsch. f. Psychol.*, 22, 1900, 321-382; W. H. Winch, Should poems be learnt by schoolchildren as 'wholes' or in 'parts'? *Brit. J. Psychol.*, 15, 1924, 64-79.

[2] *E.g.* Lakenan, *op. cit.*; H. B. Reed, Part and whole methods of learning, *J. Educ. Psychol.*, 15, 1924, 107-115; Steffens, *op. cit.*

[3] *E. g.* L. A. Pechstein, Whole versus part methods in learning nonsensical syllables, *J. Educ. Psychol.*, 9, 1918, 381-387; Pentschew, *op. cit.*; Steffens, *op. cit.*

[4] R. W. Brown, A comparative study of the whole, part, and combination methods of learning piano music, *J. Exper. Psychol.*, 11, 1928, 235-247.

[5] J. W. Barton, Smaller *vs.* larger units in learning the maze, *J. Exper. Psychol.*, 4, 1921, 418-429; L. A. Pechstein, Whole *vs.* part methods in motor learning: a comparative study, *Psychol. Monog.*, 23, 1917, (No. 99) 1-80.

[6] M. Gopalaswami, Economy in motor learning, *Brit. J. Psychol.*, 15, 1925, 226-236.

conclusion with reference to the investigations just referred to, the particular problem of the relative efficiency of whole and part methods in the sphere of non-serial reactions is itself of importance; and this for two reasons: first, we have herein a field of learning in which the matter of relative efficiency is of consequence and to which the results obtained from work with serial reactions might not be wholly and unquestionably applicable; secondly, the relative efficiency in this field has some bearing on the explanation of the results of the experiments with serial reactions.

This latter statement may require elucidation. It may be said in general that the inferiority of a part method, when it appears, has been accounted for largely by the presumed failure therein to master the "act of connection," to use Pechstein's[7] phrase, between any part and the one next following. Indeed, not only the failure to master the proper connections, but even the actual establishment of wrong connections, as between the end of a part and its beginning, which must later be broken up and eliminated, has been supposed to occur. The conclusions reached in the texts, *e.g.* those of Woodworth,[8] Gates,[9] and Dashiell,[10] bear witness to the uniformity of opinion on this point. Again, the superiority of a progressive to a pure part-method, which has been demonstrated on every occasion when such a comparison has been made, has been attributed, as by Pechstein,[11] to the opportunity afforded in the progressive method for the practice of just those acts of connection and, of course, for the avoidance of fixation of the erroneous associations mentioned. The degree of interpretative emphasis actually laid upon these points is, if anything, understated in the comments above. For example, Pechstein writes: "It seems apparent that all the waste in part learning occurs in the act of connection."[12] And in another place he speaks of the "inherent advantages" of part methods (such as "the complete utilization of the transfer items"), and states

[7]L. A. Pechstein, Alleged elements of waste in learning a motor problem by the part method, *J. Educ. Psychol.*, 8, 1917, 303-310.

[8]R. S. Woodworth, *Psychology*, 1921, 345.

[9]A. I. Gates, *Elementary Psychology*, 1925, 366.

[10]J. F. Dashiell, *Fundamentals of Objective Psychology*, 1928, 359.

[11]Pechstein, *Psychol. Monog., loc. cit.*

[12]Pechstein, *J. Educ. Psychol.*, 8, 1917, 303-310.

that those advantages "together with the elimination of place associations, explain the universal superiority of modified part methods in motor learning."[13] Such conclusions unquestionably imply that part methods are in general more efficient, provided the difficulty consequent upon the "acts of connection" in acquiring serial reactions can be overcome.

Now it would seem that if the inferiority of a pure part method to the whole method or to a progressive part method is to be explained on the bases stated, then—for the acquisition of reactions which are throughout non-serial, which lack any definite, fixed, or even predictable sequence, which are in a measure mutually isolated from one another and with reference to which, therefore, few if any definite acts of connection are to be acquired—some form of a part method, especially a pure part method, should either prove superior to any other or, at least, should be at a less than usual disadvantage. For it seems evident that in non-serial reactions any difficulty associated with acts of connection is at a minimum, and that a part method should therefore have every opportunity to display the "inherent advantages" which it has been alleged to possess. It is therefore to determine the relative economy of whole and part methods with non-serial reactions that we have undertaken this comparison of their efficiency for learning certain types of card sorting.

Procedure

Subjects. The *S*s were 143 undergraduate men and women at Washington Square College, New York University. They were all from classes in psychology and mainly from those of the writer.

Materials. The materials used were as follows:

(1) A wooden box of 9 compartments, each compartment 4½ in. square, arranged in 3 rows of 3 each. The middle row was elevated above the front row, the back row above the middle row so that, while the height of the front of the box was 3⅞ in., that of the back was 11⅛ in. Each compartment used was labeled with some number from 1 to 10 inclusive (9 was omitted) by tacking to its rear side a flinch card bearing the appropriate digit. For the preliminary practice, however, the labels were the kings of the four playing-card suits.

(2) Packs of flinch cards, each containing 72 cards divided equally (when possible) among three to nine different denominations from 1 to 10 inclusive (9 omitted).

[13]Pechstein, *Psychol. Monog., loc. cit.*

(3) One pack of ordinary playing-cards containing 72 cards selected by chance from the ace to the ten-spot inclusive, equally divided among the four suits.

The cards in every pack were always in the chance order that ordinary shuffling would yield, except that two cards of the same number were always separated from each other in the pack by at least one card of some other number.

Methods of sorting: Instructions. The following instructions were given in typewritten form to every *S*.

(*A*) *For the whole method.* "The object of this experiment is to learn to sort as rapidly as possible a pack of 9 differently numbered cards into 9 different compartments.

"Each compartment will have upon it a number, indicating that that is the compartment into which all cards bearing that number are to be sorted. The numbers used upon the cards and the compartments are those from 1 to 10 inclusive, except that '9' is omitted.

"For the work of sorting you stand up, facing the box, at any distance convenient to you. Take the pack in your left hand (*i.e.* if you are right-handed). The box is now covered, the compartments hidden from view.

"The signal to begin sorting will be the removal of the cover from the box. Then, at once, with your right hand (if you are right-handed) throw each card, as it comes in the pack, into its proper compartment. Use any method of throwing you wish. Begin work immediately when the above signal is given; your time is taken from that moment. Work as fast as possible, but avoid errors. If you make an error, go right ahead with the next card without delay; no correction of errors is allowed. Each error is scored by adding one second to your total time; hence an occasional error makes little difference in the measure of your performance.

"The experiment proper is preceded by a preliminary sorting of ordinary playing cards to familiarize you with the general nature of the work and the handling of the cards. The playing cards are to be sorted, *not* according to number, but according to suit, into four compartments, each one marked by the king of some suit; *i.e.* all the spades are to be sorted into the compartment bearing the king of spades, etc. The method of work, timing, etc., is exactly the same as described for the sortings of the numbered cards above."

(*B*) *For the pure part method.* Between the fourth and fifth paragraphs of the instructions above the following paragraph was interpolated:

"To learn the location of the 9 different numbers on the 9 compartments, you will begin by sorting packs containing 3 differently numbered cards only into 3 different compartments. Then you will sort packs containing 3 other numbered cards into 3 other compartments; and next packs containing the remaining 3 kinds of numbered cards into the remaining 3 compartments. Finally you will receive packs containing all of the 9 differently numbered cards, for which all of the 9 compartments must be used."

(*C*) *For the combination part method.* Between the fourth and fifth paragraphs of the instructions for the whole method the following paragraph was interpolated:

"To learn the location of the 9 different numbers on the 9 compartments you will begin by sorting packs containing 3 differently numbered cards only into 3 different compartments. Then you will sort packs containing 6 different cards into 6 different compartments. Finally you will receive packs containing all of the 9 different cards, for which all of the 9 compartments will be used."

(*D*) *For the progressive part method.* Between the fourth and fifth paragraphs of the instructions for the whole method the following paragraph was interpolated:

"To learn the location of the 9 different numbers on the 9 compartments you will begin by sorting packs containing 3 differently numbered cards only into 3 different compartments. Then you will sort packs containing 4 different cards into 4 different compartments; then packs containing 5 cards into 5 compartments. And so on, until finally you will receive packs containing all of the 9 different cards, for which all of the 9 compartments will be used."

The 'signal to begin' referred to in the instruction sheets was the removal of a sheet of paper from the top of the sorting box. It was always preceded by the word 'ready' or some equivalent.

Amount and distribution of practice. Every *S*, after the preliminary sorting of one pack of playing cards, sorted 10 packs of flinch cards. The interval between successive sortings was one minute.

Scoring. Scoring was in terms of the time (sec.) taken by a stop-watch, from the giving of the 'signal to begin' to the fall in the box of the last card of the pack, plus one second for each error observed. In addition the errors were separately tabulated.

Equating of the groups. The various practice groups were roughly equated according to their speed on the preliminary sorting of one pack of playing cards. All the groups scored an average speed thereon of between 64 and 68 sec.

Motivation. As to motivation, the *S*s were told in a casual manner of their speed on each pack immediately after its sorting had been completed, and their progress relative to their own past scores was frequently commented upon. *E* made no attempt to develop any unusual degree of incentive, but since most of the *S*s appeared to consider the work as a sort of psychological test possessing personal significance to them, it seems probable that their motivation was in the main satisfactory.

Arrangement to be learned. The arrangement of the cards on the compartments, *i.e.* the arrangement to be learned, was always the same for every individual sorting or trial and for each of the various practice methods described below. It was:

2	5	1
4	7	10
8	3	6

Practice Methods

The simple card sorting. (*1*) *The whole method.* In the whole method *S* simply worked for 10 consecutive trials with the 9-compartment arrangement, sorting at each trial a pack containing 8 of each of the 9 different cards.

(*2*) *The pure part method.* Each 'part' was a single row of the above arrangement. This row might be vertical, as 2, 4, 8 or horizontal, as 2, 5, 1. The order in which these parts were practiced might be any one of the following four: left, middle, right verticals; the reverse, *i.e.* right, middle, left

verticals; upper, middle, lower horizontals, or the reverse, *i.e.* lower, middle, upper horizontals. Each kind of part (*i.e.* of row) and each order of practice was employed an equal number of times. The practice upon the parts was distributed as follows: trials 1 and 2, part 1 (*i.e.* some one row, vertical or horizontal); trials 3 and 4, part 2 (*i.e.* a second row); trials 5 and 6, part 3 (*i.e.* the third row). On trials 7, 8, 9 and 10 all three parts together (*i.e.* the whole arrangement complete) were employed. Hence *S* had 6 trials with the part method, and 4 with the whole method. Since a pack always contained 72 cards, each of the 6 trials with the single parts involved sorting 24 of each of the 3 different cards. The packs for the last 4 trials were identical with those used throughout in the whole method.

(*3*) *The combination part method.* The original parts were rows exactly as described for the pure part method, and both the kind of row and the order of practicing the rows were varied in precisely the same fashion. The practice was distributed as follows: trial 1, part 1; trial 2, part 2; trial 3, part 3. *S* had now had one trial with each of the three parts. He next proceeded to work with combinations of the parts already practiced individually, as follows: trial 4, parts 1 and 2 combined; trial 5, parts 1 and 3 combined; trial 6, parts 2 and 3 combined. Trials 7 to 10 inclusive were, as before, devoted to practice with the entire arrangement (*i.e.* to practice with the whole method). Since, as always, each trial involved sorting 72 cards, 24 of each of the three cards used were sorted in trials 1 to 3 inclusive, and 12 of each of the six cards used in trials 4 to 6 inclusive.

Comparing the three practice methods thus far described, it should be noted that when *S* commenced the 7th trial, which in all cases was with the entire arrangement, he had had, regardless of his previous method of learning, exactly the same amount of practice, namely, 48 sortings, with each individual card. For each card had been sorted as follows: in the whole method 8 times in each of 6 trials; in the pure part method 24 times in each of 2 trials; and in the combination part method 24 times in one trial plus 12 times in each of 2 trials. Hence, so far as these three methods are concerned, the *S*s at the 7th trial did not differ at all in the amount of their previous practice with each individual card, but only in the kind of practice, whether with a whole, a pure part, or a combination part method.

(*4*) *The progressive part method.* In this method the part with which *S* began was, as before, a row of 3 compartments; and this row might be, also as before, the upper or lower horizontal, or the left or right vertical. He sorted one pack of 72 cards with this part. One compartment was then added to the original 3, making a total of 4 compartments, into which he sorted a pack containing 4 different cards. Another compartment was then added and he sorted a pack containing 5 different cards; and so on, adding at each trial one compartment to the arrangement and one card to the pack sorted, until on the 7th trial, again as before, the entire arrangement and all 9 cards were employed. This method of course rendered impossible an equal amount of practice with each card previous to the 7th trial. Instead, the practice on the individual cards was distributed as follows.

No. of trial	No. of times sorted					
	3 cards of original part	4th Card	5th Card	6th Card	7th Card	8th Card
1	24					
2	16	24				
3	12	12	24			
4	10	10	10	22		
5	8	8	8	8	24	
6	7	7	7	7	7	23
Totals	77	61	49	37	31	23

The selection of the compartments and of the cards for trials 2 to 6 inclusive was spatially determined. That is, if the original three-compartment part was the upper horizontal row, then the 4th compartment (the first one added) was the first compartment, reading from left to right, in the middle horizontal row; hence, in the arrangement reproduced on page 547, it was compartment 4. The 5th compartment (the second one added) was the next one in that row, namely, compartment 7. Then compartments 10, 8 and 3 were added, one at a time, in that order, until on the 7th trial compartment 6 was finally employed. If the original three-compartment part was the lower horizontal row, then the other compartments were added in an order exactly reversing that described above; namely, 10, 7, 4, 1, 5, 2. If the original part was the left vertical row, the compartments were added in the order 5, 7, 3, 1, 10, 6. And, finally, if the original part was the right vertical row, the order of addition was the reverse; namely, 3, 7, 5, 8, 4, 2. Each of the various original parts and orders of adding other compartments was used an equal number of times.

The complex card sorting. Two kinds of sorting were employed, one the 'simple', in which, as already described, each card was sorted into the compartment bearing the number identical with that on the card, and the other the 'complex,' in which the sorting was subjected to the complication introduced by, and according to, the paragraph in the instructions reproduced below. This paragraph was substituted for the first sentence in the second paragraph of the instructions for the simple card sorting already given.

"Cards bearing some particular number, as 4 for example, are not, however, to be sorted into the compartment bearing that same number. Instead the following procedure is to be adopted:

"All cards bearing the number 2 go into the compartment numbered 1, 3 into 2, 4 into 3, 5 into 4, 6 into 5, 7 into 6, 8 into 7, 10 into 8, and 1 into 10."

With the exception of this different method of sorting, the complex card sorting was identical with the simple in all the details of material and procedure heretofore described.

There was introduced, however, in the complex card sorting a modification of the combination part method which, in the presentation of results, will be termed *Combination Part Method B.* This form of the combination method differed from the one previously described in the composition of the various parts. In all the part methods thus far discussed each part consisted of a single row of the arrangement. Hence the choice of the individual cards that should constitute a part was spatially determined, and only by chance would the cards of each part bear to each other any but a spatial relation. In the

complex card sorting, however, *E* attempted to select the cards for the three separate parts in such a way that the particular difficulties encountered in this type of sorting would receive more practice than a merely spatial selection of parts would be likely to afford. Observation has repeatedly shown that the confusion which nearly all *S*s experienced was largely a confusion in the reactions to numbers in immediate sequence; for example, disturbance due to the difficulty of inhibiting the reaction of sorting a card numbered 2 into the compartment labeled 2 and of substituting therefor the less natural reaction of sorting card 2 into compartment 1; and frequently the additional difficulty of inhibiting the reaction of sorting a card into a compartment whose number was one above instead of one below it, *e.g.* of sorting card 2 into compartment 3. Especially difficult was the selection of compartment 8 as the one for card 10, and of compartment 10 for card 1. To permit more practice with these particular difficulties, the parts were composed, in this Combination Method B, not of compartments in a row in the arrangement, but of compartments in immediate numerical sequence regardless of their spatial position. Part 1 was therefore composed of the compartments labeled 1, 2 and 3; Part 2 of compartments 4, 5 and 6; and Part 3 of compartments 7, 8 and 10. Hence the cards in the pack to be sorted were as follows: for Part 1, cards 2, 3 and 4; for Part 2, cards 5, 6 and 7; for Part 3, cards 8, 10 and 1. The parts were always composed of the same compartments, but two orders of practice were employed. In one the order was the order of parts given above, in the other it was the reverse of that order. Each order was employed an equal number of times. The distribution of practice was exactly the same as that already given for the ordinary combination part method.

Results: Simple Card Sorting

Table I below gives, for all 4 practice methods, the average sorting speed and the cumulative totals of previous practice time (both in seconds) for each of the 10 trials.

TABLE I

Trial	Whole Method		Pure Part Method		Combination Part Method		Progressive Part Method	
	Speed	Total time	Speed	Total time	Speed	Total time	Speed	Total time
1	101.4	0	49.6	0	52.2	0	51.8	0
2	81.1	101.4	42.7	49.6	50.9	52.2	55.9	51.8
3	75.5	182.5	47.6	92.3	50.8	103.1	59.2	107.7
4	71.4	258.0	41.6	139.9	69.6	153.9	62.9	166.9
5	68.0	329.4	45.1	181.5	67.1	223.5	66.4	229.8
6	63.9	397.4	41.0	226.6	65.4	290.6	68.1	296.2
7	63.3	461.3	83.0	267.6	74.2	356.0	76.7	364.3
8	60.6	524.6	67.6	350.6	67.6	430.2	72.1	441.0
9	60.4	585.2	62.9	418.2	65.5	497.8	68.6	513.1
10	60.5	645.6	59.2	481.1	61.4	563.3	65.2	581.7

The table should be read as follows. On the fifth trial the average sorting speed of the whole method group was 68.0 sec. Their total practice time previous to the fifth trial was 329.4 sec. (*i.e.* the sum of their average speeds on the first four trials). The number of *S*s in each group was 16.

The following two criteria will be employed to determine the relative efficiency of the practice methods: (1) the speed attained on a given trial, *i.e.* after a given number of repetitions, a given amount of practice; and (2) the amount of practice time required to attain a given speed.

With reference to criterion (1), it must be noted that a clear and unambiguous comparison between the efficiency of the whole method, and of any part method in itself and as such, is possible only on the 7th trial, and that this trial therefore constitutes what is in a sense the *test* of the relative efficiency of the practice methods compared. In the first place, anything that could here be termed a test must of course be the speed of sorting all the cards with the entire arrangement. Secondly, the 7th trial is the part method groups' first trial with the whole arrangement. On all succeeding trials those groups have had one or more trials (of which the 7th was the first) with the whole method; *i.e.* they have practiced one or more trials with the entire arrangement, and so have become groups whose practice has been with both part and whole methods rather with a part method alone. Hence the speed on the 7th trial alone discloses the efficiency of the part methods by themselves, as preparation for the ultimate task of sorting with the entire arrangement.

An inspection of Table I will show that the average speed on this 7th trial was for the whole method group 63.3 sec., for the pure part method group 83.0 sec., for the combination part method group 74.2 sec., and for the progressive part method group 76.7 sec. Hence the whole method was superior to any part method as a preparation for work with the entire arrangement, and both the combination and progressive part methods were superior to the pure part method. The difference between the former two modified part methods was, however, very slight.[14]

Comparisons of the sorting speed attained on later trials can be made from the table. Obviously the 4 groups approach each other in speed so that by the 10th trial they are nearly equal. But it should be noted that after the 7th trial the part method groups have become mixed groups so far as their previous practice methods are concerned, so that their approximation to the speed attained by the whole method group may well be ascribed in part to their trials with the whole method.

To determine, however, which practice method was without question the most economical, these data are inadequate. For while the whole method proved most, and the pure part method least, efficient according to the criterion of speed achieved on the entire arrangement after equal amounts of practice (*i.e.* an equal number of trials), the time spent in such practice must also be considered. Inspection of the totals of practice time previous to the 7th trial shows them to have been very unequal; the total for the whole method group

[14]As to the reliability of these differences:

	Difference	S.D.
Whole—Pure Part	19.7	3.37
Whole—Combination Part	10.9	4.09
Whole—Progressive Part	13.4	3.60
Pure Part—Combination	8.8	4.14
Pure Part—Progressive	6.3	3.66
Combination—Progressive	2.5	4.33

was 461.3 sec., for the pure part method 267.6 sec., for the combination part 356.0 sec., and for the progressive part 364.3 sec. Differences of this kind permit such a question as: Is a method of learning which yields a speed of 63.3 sec. after 461.3 sec. of practice (which was the record of the whole method group) more economical than one which yields a speed of 74.2 sec. after 356.0 sec. of practice (which was the record of the combination part group)? The difficulty of answering such a question indicates that other criteria than speed on a single trial must be employed.

Let us therefore consider the relative efficiency of the four practice methods according to the second criterion mentioned above; namely, the amount of practice time required to attain various given speeds.

The selection of the 'given speeds' is necessarily somewhat arbitrary. We shall here consider the amount of practice time (and number of trials) required to attain the speeds of 75, 65 and 60 sec. only.

(a) To attain a sorting speed of about 75 sec. (Data from Table I.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	3	182.5
Pure Part	8	350.6
Combination Part	7	356.0
Progressive Part	7	364.3

Hence to attain a sorting speed of about 75 sec. the whole method was far superior to any part method. The three part methods were about equal in efficiency. But the pure part group attained the above speed only after one trial with the whole method, so that its equality with the other part method groups is somewhat doubtful.

(b) To attain a sorting speed of about 65 sec. (Data from Table I.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	6	397.4
Pure Part	9	418.2
Combination Part	9	497.8
Progressive Part	10	581.7

Hence to attain a sorting speed of about 65 sec. the whole method group was slightly superior to the pure part method group, and greatly superior to the combination and progressive part groups. Of the three part method groups the pure part was distinctly superior, the progressive part inferior. It should be noted, however, that no part method group attained such a speed as a result of practice with a part method alone, but only as a result of practice with both part and whole methods.

(c) To attain a sorting speed of about 60 sec. (Data from Table I.)

	Trial on which the above speed was approximated	Total of previous practice time
Whole	8	524.6
Pure Part	10	481.1
Combination Part	10	563.3
Progressive Part	Not attained	581.7

Hence to attain a sorting speed of about 60 sec. the pure part method was superior to the whole method group, the latter was superior to the other two part method groups, the progressive part group was inferior to all. But again it must be remembered that no part method group even approximated such a speed as a result of their practice with a part method alone. The pure part group, for example, required 4 trials with the whole method in addition to their 6 with the pure part method before they reached that speed.

The above results of the simple card sorting may be thus summarized:

(1) According to the criterion of sorting speed attained on a given trial, in particular the seventh, which may be considered to be the 'test trial,' amount but not time of practice being constant, the whole method was superior, the pure part method inferior, the combination and progressive part methods intermediate in efficiency.

(2) According to the criterion of the amount of practice time required to attain a given speed, the whole method was in general superior, especially for the attainment of the lower speeds. The combination and progressive part methods were throughout inferior to the whole method. The pure part plus whole method was superior to any part plus whole method for the attainment of the higher speeds.

Results: Complex Card Sorting

Table II gives, for all 5 practice methods, the average sorting speed and the cumulative totals of previous practice time (in sec.) for each of the 10 trials. In addition Table III below gives the number of errors made by each group on trials 7 to 10 inclusive.

As was the case with the simple card sorting, two criteria will be employed to determine the relative efficiency of the whole and of the four part methods; namely, the speed attained on a given trial and the amount of practice time required to attain a given speed.

As before, the sorting speed on the 7th trial must be considered as the test, the only unambiguous way of applying the first criterion. An inspection of Table II will show that the average speed on this trial was for the whole method group 85.6 sec., for the pure part method group 146.3 sec., for the combination part 108.2 sec., for the combination part B 106.2 sec., and for the progressive part 102.6 sec. Hence the whole method was without question

TABLE II

Trial	Whole Method		Pure Part Method		Comb. Part Method		Comb. Part Method B		Prog. Part Method	
	Speed	Total Time	Speed	Total Time	Speed	Total Time	Speed	Total Time	Speed	Total Time
1	146.4	0	65.2	0	63.4	0	67.7	0	70.1	0
2	119.2	146.4	49.7	65.2	65.9	63.4	72.9	67.7	68.7	70.1
3	104.1	265.6	66.8	114.9	58.6	129.3	80.2	140.6	76.7	138.8
4	98.9	369.7	49.9	181.7	102.7	187.9	106.0	220.8	89.8	215.5
5	89.8	468.6	59.1	231.6	98.6	290.6	112.2	326.8	95.4	305.3
6	91.7	558.4	46.0	290.7	95.7	389.2	95.1	439.0	99.7	400.7
7	85.6	650.1	146.3	336.7	108.2	484.9	106.2	534.1	102.6	500.4
8	82.7	735.7	111.9	483.0	97.0	593.1	93.6	640.3	92.9	603.0
9	80.1	818.4	101.7	594.9	86.4	690.1	87.1	733.9	91.6	695.9
10	76.2	898.5	93.9	696.6	83.7	776.5	84.0	821.0	85.3	787.5

The table should be read as follows: On the 5th trial the average of the whole-method group was 89.8 sec. The total practice time previous to the 5th trial was 468.6 sec. (*i.e.* the sum of the average speeds on the first 4 trials). The number of *Ss* in each group was 16, except in the pure part method group, where it was 15.

TABLE III

Total Number of Errors Made by Each Group

Trial	Whole Method	Pure Part Method	Comb. Part Method	Comb. Part Method B	Prog. Part Method
7	10	116	52	43	18
8	11	41	43	30	22
9	10	39	18	28	16
10	6	15	18	17	16

superior, the pure part method inferior, the other three part methods intermediate in value and about equal to each other, as preparation for work with the entire arrangement.[15]

Comparisons of the speeds attained on later trials can be made from Table II. The whole method group remains throughout superior, the pure part group inferior, the other part method groups intermediate and about equal to each other. Apparently the practice from the 7th trial on with the entire arrangement, which all the part method groups of course had, did not enable them even to approximate the speed of the whole method group.

As we saw in the case of the simple card sorting, however, the amount of time spent in practice previous to any given trial was very unequal for the several groups. Hence the second criterion, that of the amount of practice time required to attain various sorting speeds, should be applied. The speeds chosen for such comparison are 105 and 85 sec.

(a) To attain a sorting speed of about 105 sec. (Data from Table II.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	3	265.6
Pure Part	9	594.9
Combination Part	7	484.9
Combination Part B	7	534.1
Progressive Part	7	500.4

Hence to attain a sorting speed of about 105 sec. the whole method was by far the superior, the pure part method inferior, the other part methods intermediate in value and about equal to each other. With respect to the pure part method groups, it may be further noted that they attained the above speed only after a practice with the entire arrangement amounting to 2 trials and 258.2 sec.

(b) To attain a sorting speed of about 85 sec. (Data from Table II.)

Group	Trial on which the above speed was approximated	Total of previous practice time
Whole	7	650.1
Pure Part	Never attained.	
Combination Part	9	690.1
Combination Part B	9	733.9
Progressive Part	10	787.5

[15]As to the reliability of these differences:

	Difference	S.D.
Whole—Pure Part	60.7	10.49
Whole—Combination Part	22.6	4.85
Whole—Combination Part B	20.6	7.50
Whole—Progressive Part	17.0	5.92
Pure Part—Combination	38.1	10.68
Pure Part—Combination B	40.1	11.69
Pure Part—Progressive	43.7	10.75
Combination—Combination B	2.0	7.76
Combination—Progressive	5.6	6.24
Combination B—Progressive	3.6	7.86

Hence to attain a sorting speed of about 85 sec. the whole method group was superior, the pure part group inferior. The other part method groups were intermediate in attainment; their efficiency relative to each other is obvious from the table.

The error scores shown in Table III should also be noted. It will be seen that the number of errors made by the part method groups when working with the entire arrangement greatly exceeded those made by the whole method group; and further, that the errors made on the 7th trial by the pure part group greatly exceeded those made by any other group on that trial. In fact they even exceeded the number (which was 102) made by the whole method group on their first trial.

The results of the complex card sorting may be thus summarized. According to the criteria both of the sorting speed attained on a given trial, especially the 7th, and of the amount of practice time required to attain given sorting speeds, the whole method was superior, the pure part method inferior. The other three part methods were intermediate in efficiency, and were about equal to each other. These differences were much greater and more consistent than they were with the simple card sorting.

Discussion of the Results

The results of the experiments with simple and complex card sorting may be stated as follows.

(1) The whole method was in general superior to any part method for learning the non-serial reactions involved.

(2) Of the several part methods employed the pure part, in which there was no practice with parts in combination previous to work with the entire arrangement, was in general the least advantageous.

(3) The various forms of part methods in which such practice of parts in combination or progression occurred were intermediate in efficiency between the whole and the pure part method. Since such part methods resemble a whole method more than a pure part method does, their superiority to the latter serves if anything to lend support to the first two conclusions above.

(4) All of the conclusions above apply even more clearly and unquestionably to the complex than to the simple card sorting.

The general applicability of these conclusions to the learning of all non-serial reactions is of course doubtful. The present experiments dealt with one type of non-serial reaction only, that

involved in card sorting. Further, the card sorting was on the whole of a rather simple kind; only a small number of different cards and compartments was used, the total amount of practice was very brief, and in the part methods the parts and the number of trials allotted to each part were arbitrarily chosen. It is possible that greater complexity of reaction, a larger number of cards, a different amount of total practice, a different selection of or a different amount of practice on the parts might have favored, even in the field of card sorting itself, some form of a part method. Nevertheless the results of the experiments appear, with due regard to the limitations stated, to be valid and to justify some attempt at explanation.

There are, we believe, two aspects of the results which in particular require accounting for; (1) the inferiority of any part method to the whole method; and (2) the greater inferiority of part methods in the complex than in the simple card sorting.

With reference to the question as to why the part methods were inferior to the whole method, the following explanations among others are theoretically possible.

(*a*) *Negative transfer.* It is possible that negative transfer from one part to a succeeding one occurred. Inspection of Tables I and II, however, shows clearly that transfer is, if anything, positive in most cases. For example, the sorting speed of the pure part method group is greater on trial 3, its first trial with part 2, than it is on trial 1, its first trial with part 1; and likewise is greater on trial 5, its first trial with part 3, than it is on trial 3. Pechstein, it may be noted, also found only positive transfer from one section of his maze to another.[16]

(*b*) *Retroactive inhibition.* It is possible that practice on one part might impair retention of parts previously practiced. We have no data on this point; but Pechstein found no evidence of such inhibition in learning mazes by the part method.[17]

(*c*) *Forgetting.* The lapse of time between the completion and the resumption of practice with a part might impair retention thereof. This possibility is most evident in the pure part method where, for example, *S* after his initial trials with Part 1 did not encounter the cards and compartments of that part again

[16]Pechstein, *loc. cit.*
[17]Pechstein, *ibid.*

until he reached the entire arrangement on the 7th trial. It seems improbable, however, that retention was significantly affected from this cause; for the interval of time between such completion and resumption of practice was in all cases very short, and in the progressive part method it was merely the constant interval between all trials by any method, namely, one minute.

(*d*) *The 'ease' of the parts.* In both the simple and the complex card sorting the mastery of the parts seemed very easy. By the second trial with each part (in the pure part method), or even before the end of the first, the sorting had become largely automatized and proceeded with great speed and few if any errors. The work on the parts may therefore have been so easy that it failed to stimulate a degree of incentive, effort, concentration adequate for maximum efficiency when work with the entire arrangement was begun. Or it may have occasioned in the *S*s a false notion as to the difficulty of their task as a whole, a premature over-confidence and over-expectation, which was a definite hindrance when they came to the much harder work of sorting with the entire arrangement. Such factors may have had some deleterious influence; but we have no data to show whether or not they did have.

(*e*) *Massed vs. distributed practice.* In all this card sorting the practice may be classified as having been massed rather than distributed, since the interval between trials was always one minute only and the entire series of trials was in most cases completed within a half-hour. In his monograph on maze learning, Pechstein points out that the whole method was at a special disadvantage when practice was massed.[18] He further remarks that "he is inclined to argue from his results to the general motor field," within the limits of which card sorting surely lies, and likewise that "under massed conditions the part methods are always more efficient." Had part methods been so in the present experiment, we might have utilized the above conclusions as a partial explanation thereof. But since our results run counter to Pechstein's generalizations, we would suggest rather that the latter need some sort of qualification or amendment.

In a later article, however, Pechstein relates the double problem of part *vs.* whole methods on the one hand and massed *vs.*

[18]Pechstein, *ibid.*

distributed practice on the other to a third factor, namely, the difficulty of the problem, and in this connection writes: "The longer and more difficult the problem, the more advisable to break it up into units and learn both the units and the connection of these under massed conditions . . . The hard problem becomes easy if it is learned under massed conditions by the part method and in no other way; it remains hard if it is learned as a whole under massed or distributed conditions, or even in parts under distributed conditions."[19] It might be inferred from the above that with easy problems Pechstein would expect the combination of massed practice and whole method to be less disadvantageous. Since both the simple and the complex card sorting employed in our experiments are, we presume, to be classified as easy problems, our finding the whole method superior to any part method even when practice was massed is perhaps less contrary to his conclusions than might at first appear. Nevertheless, if we take his generalizations literally, we would at least expect to find the whole method of less advantage in the complex card sorting, which was the harder problem learned under massed conditions, than it was in the simple, which was much the easier problem, provided, that is, that other things were equal. One can not of course assume that other things were equal; but one may perhaps suggest that the whole method can not be so decisively inferior for the learning of a harder problem under massed conditions as Pechstein's conclusions would imply.

(*f*) *Relationships among the reactions practiced.* We have already pointed out that failure to connect the parts properly is the particular disadvantage from which a pure part method is supposed to suffer and which some sort of progressive or combination part method is supposed, usually if not always, to eliminate. We have further indicated that in non-serial reactions this disadvantage should be at a minimum and that the learning of such reactions should therefore provide a field especially favorable to a part method. In card sorting the reactions are certainly not serial; they follow no invariable sequence; they do not constitute *in toto* an integrated closely ordinated whole. Nevertheless it would be going too far to claim that these non-serial reactions were wholly isolated from, and independent of, each other. (1) For one

[19]Pechstein, *J. Educ. Psychol.*, 12, 1921, 92-97.

thing, an important phase of the work was learning to find rapidly the particular compartment into which any given card was to be sorted, a process which required selection and fixation of the appropriate eye- and the coördinated hand-movements. It is quite conceivable that this process was assisted by observation of the numerical and spatial relationships between differently numbered compartments, *e.g.* by seeing that the three compartments on the left were 2, 4 and 8, all of which are even numbers; or that 2 and 1 occupy the two upper corners but in a left-right sequence reversing their order in the number series; or that 7 was to the right of four, 10 just below 1, 4 just above 8. Such observations were made by the *S*s; they may have been of importance to rapid learning; they may have been made to better advantage when there was throughout a visual presentation of all, or a greater part of, the nine numbers on the nine compartments. Hence the whole method may have favored their acquisition and the part methods, especially the pure part part method, been unfavorable thereto. (2) Another factor of possible significance was the development of more rapid and accurate discrimination between one card and another as they appeared in the pack; *e.g.* between 3 and 8, which some *S*s found confusingly similar. Such discrimination might be facilitated by the use of packs containing all nine cards, that is, by those used throughout in the whole method. There is no evidence to suggest, however, that this factor was of any great significance. With respect, then, to this matter of relations, we are of the belief (1) that although the reactions practiced were non-serial, they were in actual fact brought, in some measure, into relation with each other by the *S*s; certainly it seems improbable that any group of responses practiced together ever could remain wholly discrete one from the other; and (2) that the establishment of such relationships may have been important to learning and have been favored by practice with the whole method.

The final conclusion with reference to the first problem stated earlier, namely, the reason for the rather consistent inferiority of part methods and especially of the pure part method, to the whole method must unfortunately be that no completely satisfactory explanation is at hand, unless indeed the above factor of establishing relations be considered adequate. The writer expected to

find part methods greatly superior to whole methods in learning the non-serial reactions of card sorting. This expectation was not realized, and for the contrary results we can not fully account at present. But the fact that the results did not accord with a legitimate expectation does, we believe, justify these two conconclusions. (1) That since the above described relations were not serial connections, one can not state as a universally applicable law that *all* the waste in part learning occurs in the act of such connection; and (2) that since no form of combination or progressive method proved equal in efficiency to the whole method, one likewise can not assert as a universally applicable law that such modifications of the part method, in which the above-mentioned waste would presumably be avoided, must be expected *always* to be superior to the whole method.

The second problem cited earlier for explanation was the greater superiority of the whole method and the greater inferiority of the pure part method in the complex card sorting. The principal differences between the simple and the complex sorting seem to be the greater difficulty of the latter, and the potentially greater number and complexity therein of the relations between the various reactions to be learned.

With reference to the first difference above there is little additional to be said. A part method might be expected to be more advantageous in the harder of two problems learned by a massed method, but it was as a matter of fact less so. With reference to the second difference, however, some analysis is possible. We have already noted that in the simple card sorting the various reactions, though certainly not serial, were nevertheless not wholly independent of each other. In the complex sorting they were probably even less discrete. For in this type of sorting not only do all the relationships mentioned as present in the simple card sorting exist, but also, as was pointed out on page 550, an important phase of the learning was the acquisition of certain inhibitory reactions which were not demanded in the simple card sorting. Among these was, for example, learning to inhibit the reaction of sorting card 6 into compartment 6, a response more "natural" than that of sorting 6 into compartment 5, which was the act required; or inhibiting the reaction of putting card 6 into compartment 7, which is the compartment numerically one

above instead of one below the card to be sorted. Since it is generally agreed that a response, whether classified as inhibitory or not, can not be acquired unless it is in some sense made, the most satisfactory development of these inhibitions would require that the reaction to be inhibited be itself made or in some manner initiated; and this in turn would require that its stimulus be present. Therefore to provide the maximum possibility of learning all the inhibitions potentially involved, it would seem advantageous to have had all the compartments presented with their proper numbers and so open to visual observation and to the chance of erroneous response to them. This condition, however, was met completely only in the whole method and was satisfied least adequately in the pure part method. Certainly some factor was present in the complex sorting which decisively favored the whole method. And if that factor was this greater complexity of relationships, then the differences in the results of the simple and the complex card sorting are perhaps explained. But since these relationships were not serial connections, and since even in the complex sorting neither combination nor progressive part methods proved equal in efficiency to the whole method, the analysis here merely lends additional weight to our previous criticisms of existing generalizations concerning the sole source of waste in part methods and the uniform desirability of a combination or a progressive rather than a whole method.

Summary

The purpose of the experiment was to compare the efficiency of the whole and of three types of part method, a 'pure,' a 'combination,' and a 'progressive,' for learning certain non-serial reactions, namely, those involved in two types of card sorting, one simple, the other relatively complex. The criteria of efficiency were (a) the sorting speed attained on some given trial with the 'whole,' *i.e.* with all the cards and compartments used; and (b) the amount of practice time required to attain some given speed therewith. According to both criteria the whole method was in general superior, the pure part method inferior, the combination and progressive methods intermediate in efficiency and about equal to each other. The above differences were greater and more consistent for the complex than for the simple card sorting. Since experiment and theory in the field of

serial reactions have suggested that the inferiority of part methods, especially of a pure part method, when found is due to the failure therein to give practice to the act of properly connecting certain responses with succeeding ones, it would be expected that in non-serial reactions, where there are no such connections to be acquired, part methods, and especially a pure part method, would possess an advantage far greater than common. The fact that all the part methods, even those of combination and progressive types, were definitely inferior for learning card sorting under the experimental conditions employed indicates: (1) that the crucial reason for the inferiority of a part method need not in all cases reside in this matter of serial connections (even though it may in part be found in failure to master relations other than serial); (2) that a combination or progressive part method is not necessarily superior to a whole method, even in the field of motor learning; and (3) that in consequence existing generalizations to such effect stand in need of qualification.

THE RELATIVE RÔLES OF THE TEMPORAL AND THE INTENSIVE FACTORS IN SOUND LOCALIZATION[1]

By Otis C. Trimble, Bradley Polytechnic Institute

I. Introduction

The outstanding problem, as indicated by current theories,[2] in the field of auditory localization is the determination of the relative rôles of the different factors that condition directional localization. Some investigators hold that localization is a function of the binaural integration of a single factor, either phase, time, or intensity, and we have the corresponding theories, viz., the phase-theory, the time-theory, and the intensity-theory. Others hold that phase and time are reducible to intensity, which gives us a further interpretation of the intensity-theory. Still others maintain that all of the factors are effective as such, but they would limit the effectiveness of each factor to a certain frequency range, phase being effective in the low range, intensity in the very high, while all three of the factors are more or less equally effective in the intermediate frequency range. This may be called a phase-time-intensity-theory. The writer believes that these three factors, and possibly also mass, contribute in a varying degree, depending upon the conditions, to a binaural difference pattern that results in directional localization. This may be called a 'difference-pattern-theory.' All of these theories and interpretations are supported by experimentation; but it seems that so much theorizing serves only to define the problem of the specificity of the stimuli more clearly, and to indicate that this problem requires much more, and perhaps more systematic, experimentation.

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[1] From the Psychological Laboratory, State University of Iowa.

[2] The following review the theory of sound localization: C. E. Ferree and R. Collins, An experimental demonstration of the binaural ratio as a factor in auditory localization, this Journal, 22, 1911, 250-297; H. M. Halverson, Binaural localization of tones as dependent upon differences of phase and intensity, *ibid.*, 33, 1922, 178-212; E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, *ibid.*, 37, 1926, 157-188; C. A. Ruckmick, Recent acoustic research, *Psychol. Bull.*, 21, 1924, 617-621; also, Auditory sensations and related phenomena, *ibid.*, 24, 1927, 81-99; also, Facts and theories of audition, *ibid.*, 25, 1928, 229-244; O. C. Trimble, The theory of sound localization: a restatement, *Psychol. Rev.*, 35, 1928, 515-523.

The present investigation has approached the problem in a series of experiments dealing with the temporal and the intensive factors. We wished to determine which factor, if either, is the more effective in conditioning directional perception.

Previous investigations have dealt more or less directly with this particular problem. Klemm found that an intensive difference of a 3 to 1 ratio could be equalized by a temporal difference;[3] *i.e.* the phantom sound localized toward the ear receiving the more intense stimulus could be brought to the median plane by gradually increasing the temporal disjunction so that the ear receiving the weaker stimulus was the ear first stimulated. He reports, however, that the phantom remained on the side of the stronger stimulus during the smaller temporal differences, and that it tended to flash over to the earlier stimulated ear during the larger temporal differences. He reports also that localization was more indefinite and uncertain under these conditions.

Von Hornbostel and Wertheimer report that when the phantom sound was localized toward the side of the ear receiving the prior stimulus, if the prior stimulus was gradually weakened, the angular displacement of the phantom sound remained unchanged until the prior stimulus-sound was completely blotted out, at which time the phantom jumped suddenly over to the other side.[4]

Wittmann apposed intensive differences with a time-difference, and temporal differences with an intensity-difference, and found that both factors could supplement or neutralize each other to a varying degree in their effect upon localization.[5]

Banister[6] apposed phase differences[7] with intensive differences and found that a difference of phase at the ears might cause a displacement of the phantom sound even when the intensive differences were relatively very large. In another study this investigator apposed intensive differences with a phase difference of 90° and found that it took a binaural intensive ratio of at least 2.5:1 to produce the same angular displacement of a tone of 220-300 d.v. as did the phase difference.[8]

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[3]O. Klemm, Über den Einfluss des binauralen Zeitunterschiedes auf die Lokalisation, *Arch. f. d. ges. Psychol.*, 40, 1920, 117-146.

[4]E. M. von Hornbostel und M. Wertheimer, Über die Wahrnehmung der Schallrichtung, *Sitzungsb. d. preuss. Akad. d. Wissensch.*, 1920, 388-396.

[5]J. Wittmann, Beiträge zur Analyse des Hörens bei dichotischer Reizaufnahme, *Arch. f. d. ges. Psychol.*, 51, 1925, 21-122.

[6]H. Banister, A preliminary note on a new method of determining the phase effect in the localization of sound, *Brit. J. Psychol.*, 13, 1923, 435-436.

[7]Phase-difference may be considered as a special case of time-difference. Cf. G. W. Stewart, The intensity logarithmic law and the difference of phase effect in binaural audition, *Psychol. Monog.*, 31(1), 1922, 41; E. G. Boring, *op. cit.*, 166 ff.; H. M. Halverson, *op. cit.*, 200; O. C. Trimble, The phase-theory as a time-theory in sound localization, *Proc. Iowa Acad. Sci.*, 24, 1922, 286.

[8]H. Banister, Three experiments on the localization of tones, *Brit. J. Psychol.*, 16, 1926, 265-292.

Halverson found that large differences in the intensive ratio were required to effect even small changes in localization conditioned by phase differences; and that with intensive differences localization varied only slightly, except with very large differences in intensity, under which conditions double lateral localization resulted.[9] He concluded that localization with intensive differences may be discontinuous and not uniform as is the case with phase differences.

This review of previous studies shows that the findings are more or less contradictory; but they indicate that time may be the more effective factor in determining localization.

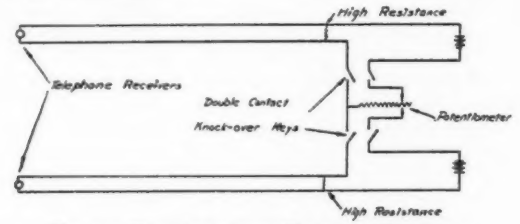

FIG. 1. DIAGRAM OF THE ELECTRIC CIRCUITS

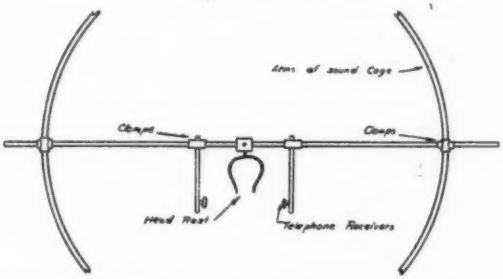

FIG. 2. DIAGRAM OF THE RECEIVER SYSTEM

II. PROBLEMS, PROCEDURES, AND RESULTS

A discrete impulse technique was employed in the present investigation.[10] The stimulus-sounds were generated by breaking electric currents (see Fig. 1) through telephone receivers of identical type. The receivers were fixed in a system (see Fig. 2) attached to the swinging arms of a Seashore sound cage,[11] so that they were kept at a constant distance, 6 cm. from the ears. The re-

[9]Halverson, *op. cit.*, 203 ff.

[10]The technique was a modification of the discrete impulse technique which has been described in detail in a previous article. O. C. Trimble, A discrete impulse technique in sound localization, *Brit. J. Psychol.*, 19, 1928, 167-178.

[11]C. E. Seashore, A sound perimeter, *Psychol. Rev.*, 10, 1903, 64-68.

ceiver membranes were adjusted so that they generated high-pitched sounds, because localization with low-pitched sounds proved difficult. The open air method of delivering the stimuli was employed, which made it necessary to use very weak sounds in order to avoid the second phantom sound[12] which is caused by the diffraction of sound about the head.

The temporal factor was controlled by means of a pair of double knock-over contact keys, the relative positions of which could be varied by changing the set-screws. The series of time-differences were presented in terms of sigma. A Dunlap pendulum[13] was used to throw the contact keys, thus breaking the circuits. By means of a potentiometer with which the resistance in one circuit could be decreased while the resistance in the other circuit was increased, the intensive factor was controlled. The intensive series was presented in terms of δ, the ratio of the strength of the currents (milliamperes) passing through the receivers at the different points.[14]

Observers. The *Os*, all graduate students in psychology, had served in a previous investigation of considerable length, in which a discrete impulse technique, much the same as that used in the present study, was employed.[15] The *Os* were seated, singly, in a sound-proof room which was darkened a few seconds before and during each presentation of the stimuli. They were instructed to record the direction and the angular displacement of the sounds. Ascending and descending series were presented on both sides, and in such a way that the *Os* could not follow any preconceived plan.

Problems. With this technique, the problem was attacked from five different angles: (*1*) localization as a function of intensive differences; (*2*) localization as a function of temporal differences; (*3*) localization with intensive differences apposed with a temporal difference; (*4*) localization with temporal differences

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[12]This phenomeon was discovered in a previous investigation. O. C. Trimble, Some temporal aspects of sound localization, *Psychol. Monog.*, 38, 1928, (no. 1), 172-225.

[13]K. Dunlap, A new laboratory pendulum, *Psychol. Rev.*, 19, 1912, 240-245.

[14]A ratio of 6.57, for example, would mean an intensive ratio of 43.16 at this particular point in the series, if the laws of physics were applicable: viz., (1) that the amplitude of the vibration of the receiver diagram is directly proportional to the strength of the current passing through the receiver, and (2) that the acoustic energy in the sound wave is directly proportional to the square of the amplitude. The writer believes that these laws do not absolutely hold in this case because (1) the receiver membranes were weighted on both sides with heavy cardboard washers (thus limiting the vibrating surfaces so that they generated high-pitched sounds), which doubtless interfered with the force of induction. Also, (2) since receiver membranes are practically 'deadbeat,' a single swing of the membrane may not have been translated into acoustic energy to the degree that a continuous, uniform vibration is. These conditions were so complex that it was decided best to let the intensive series stand in terms of the ratio of the strength of the currents; but no doubt the binaural intensive ratio in the last part of the series was very much greater than that which results from the head-shadow in normal conditions of hearing.

[15]O. C. Trimble, *op. cit.*, *Psychol. Monog.*, 183 ff.

apposed with an intensive difference; and (*5*) localization with both factors varied together in the same direction.

(*1*) *Localization as a function of intensive differences.* The first experiment attempted to determine whether localization on the basis of the intensive factor is discontinuous as has been suggested by Halverson,[16] or whether it is continuous and uniform as was found by Stewart and Hovda.[17] These investigators employed continuous sounds, namely tones, as stimuli, but they controlled the intensive factor by different means. Halverson used a rheostat with which the resistance in one receiver circuit could be increased while the resistance in the other circuit was decreased. Stewart and Hovda controlled the intensity of the stimuli by varying the distance of the receiving tubes from the sound source. This method introduced a phase difference which the investigators held was so slight as to be negligible, but it may have been sufficient to bridge the gap which Halverson found.

Results. Table I presents the data, from which it is evident that angular displacement is continuous and more or less uniform. The estimates for all of the *O*s, except *M*, gradually increase as the intensive ratio increases, until maximal lateral localization is reached. For *M* the threshold of localization is not reached until the binaural ratio is relatively large. After this threshold is reached the phantom changes rapidly to the maximal lateral position. In order to determine whether *M*'s was a case of somewhat discontinuous localization, a series of much finer intensive differences ranging above his localization threshold was offered, and his estimates showed a gradual and more or less regular increase. The average deviations show that localization is more or less consistent for each of the *O*s. Localization near the medial and the maximal lateral positions seems to be more consistent than it is at intermediate points.

The *O*s described the course of the phantom sound as corresponding somewhat to a semi-circle, a 90° arc on either side of the median plane.

It appears, therefore, that localization on the basis of intensive differences, when discrete sounds are used as stimuli,[18] is continuous and more or less uniform.

[16]H. M. Halverson, *op. cit.*, 210.

[17]G. W. Stewart and O. Hovda, The intensity factor in binaural sound localization: an extension of Weber's law, *Psychol. Rev.*, 25, 1918, 242-251.

TABLE I

LOCALIZATION AS A FUNCTION OF INTENSIVE DIFFERENCES

Intensive ratio	Observers			
δ	*H*	*Hd*	*L*	*M*
1.00	X*	X	X	X
1.13	1	4	1	X
1.27	3	10	4	1
1.42	5± 3	14±11	7± 4	1± 2
1.60	7± 3	19±17	11± 4	2± 3
1.80	12± 4	29±18	15± 4	3± 4
2.03	15± 7	38±18	21± 4	2± 3
2.28	21± 8	47±21	24± 4	3± 4
2.57	27± 8	54±17	28± 4	5± 4
2.87	35±11	62±20	34± 5	7± 6
3.25	45±14	68±17	39± 6	14±12
4.11	59±15	73±14	47± 5	20±19
5.20	73±10	77±10	56± 6	34±21
6.57	86± 4	82± 6	66± 5	48±25
8.32	88± 2	85± 1	74± 5	73±17
10.53	90± 0	87± 2	81± 4	79±11
11.84	90± 0	89± 1	85± 1	88± 4

*The numbers given in this table are averages, with average deviations, of approximately 10 estimates, by the individual *Os*, of the angular displacement of the phantom sound from the median plane, at the different points in the intensive series. X represents an unknown because of slight errors in localization. When no average deviation is given, the number represents an estimated average, because of errors in localization.

(*2*) *Localization as a function of temporal differences.* Localization on the basis of the temporal factor has been described in a previous article;[19] but in order to establish comparable units of angular displacement, and to make direct comparison possible, localization with time-differences was determined under the conditions that obtained in the foregoing experiment.

Results. Table II summarizes the results. A comparison of this table with Table I shows that angular displacement with intensive differences increases more rapidly on the basis of the intensive increment employed than does angular displacement with time-differences on the basis of the temporal increment employed. Otherwise the localization in both cases is essentially the same, except for the further fact that in localization on the basis of the temporal factor double lateral localization was reported before the end of the series, at approximately 2σ.

[18]Continuous sounds are so complex, due to diffraction of sound about the head, and to auditory persistence, summation, and fusion, that the present writer doubts if any of the factors under consideration can be adequately controlled under such conditions.

[19]O. C. Trimble, *op. cit.*, *Psychol. Monog.*, 220 ff.

TABLE II

LOCALIZATION AS A FUNCTION OF TEMPORAL DIFFERENCES

Temporal differences	Observers			
σ	*H*	*Hd*	*L*	*M*
000	X	X	X	X
.12	4	9	6	6
.24	7± 1	17± 6	9± 1	10± 5
.36	11± 7	28± 6	13± 2	15± 8
.48	17± 5	31± 5	13± 2	22± 7
.60	21±10	35± 8	15± 2	30± 9
.72	29±10	29± 7	17± 3	35±14
.84	37±11	42± 6	17± 2	45±16
.96	44±12	44± 7	20± 4	58±15
1.08	49±12	50±10	21± 3	64±19
1.20	57±14	50±10	23± 6	65±24
1.32	66±15	54±11	37± 8	72±25
1.44	72±16	54±11	39± 5	72±21
1.56	72±18	58±14	39± 5	70±16
1.68	74±14	52± 6	40± 4	72±17
1.80	79±11	53± 7	41± 4	76±17
1.92	82± 8	58± 9	43± 6	72±14
2.04	86± 3	61±11	49±15	74±19
2.16	87± 3	63± 9	53±14	70±20
2.28	88± 1	65± 8	54±18	64±19
2.40	88± 1	66± 9	55±10	68±22
2.52	90± 0	65± 9	58±10	68±22
2.64	90± 0	65± 9	63± 9	76±17
2.76	90± 0	65± 9	67± 8	80±12
2.88	90± 0	70±10	73± 7	82± 9
3.00	90± 0	70±10	78± 5	86± 6
3.12	90± 0	75±10	81± 5	88± 3
3.24	90± 0	76±11	84± 3	90± 0

The temporal and the intensive series may be considered as fairly well balanced, since the fused sound divides at about the same time in the temporal series that the phantom sound comes to the maximal lateral position in the intensive series.

These series, or units of these series, were employed in the remaining experiments of this analysis.

(*3*) *Localization with intensive differences apposed with a temporal difference.* The third experiment of the series dealt with the effectiveness of intensive differences apposed with a relatively large temporal difference. The purpose was to determine the effect on a localization conditioned by a time-difference of presenting the stimuli so that the later stimulus was also the stronger stimulus. The stimuli were presented with a temporal disjunction of 1.92σ in order to obtain as nearly as possible a maximal lateral localization without the double lateral effect. It was thought best to avoid the double lateral effect because under such condi-

TABLE III

LOCALIZATION WITH INTENSIVE DIFFERENCES APPOSED WITH A RELATIVELY LARGE TIME-DIFFERENCE

Intensive ratio	Observers							
	H		*Hd*		*L*		*M*	
δ	I	T	I	T	I	T	I	T
1.13	90± 0	85±10	—	74±13	—	63± 9	—	69±21
1.27	90± 0	89± 1	—	61± 9	—	59±13	—	68±27
1.42	90± 0	90± 0	—	49±13	—	36± 9	—	58±25
1.60	90± 0	90± 0	—	43±16	—	42±17	90± 0	56±15
1.80	90± 0	90± 0	—	37±21	17± 2	35±14	90± 0	41± 4
2.03	83± 3	90± 0	—	25±17	17± 2	18± 5	84± 9	37± 9
2.28	89± 1	83±11	?	16±17	20± 0	18± 3	85± 7	52±18
2.57	77±18	75±15	?	17±17	22± 2	17± 5	79±16	31± 9
2.87	90± 0	78±18	35±16	?	20± 3	18± 3	90± 0	28± 9
3.25	90± 0	79±18	47±22	?	25± 5	13± 5	72±21	26±12
4.11	90± 0	66±23	?	?	26± 4	15± 2	78±19	30± 4
5.20	90± 0	58±32	47± 2	—	26± 4	15± 4	85± 9	36±22
6.57	90± 0	62±28	70±13	—	32±13	15± 4	90± 0	46±19
8.32	90± 0	?	87± 4	—	33±14	?	81±14	?
10.53	90± 0	63± 9	87± 4	—	58±25	—	90± 0	—
11.84	90± 0	—	88± 3	—	76±14	—	90± 0	—
12.32	90± 0	?	89± 1	—	85± 5	—	90± 0	—

These numbers are averages, with average deviations, of 5 estimates, by the individual observers, of the angular displacement at the different points in the intensive series.

— indicates that sound was not reported.

? indicates that the *O*s were uncertain whether they heard the sound or not, or that they occasionally reported double lateral localization.

tions the influence of the intensive factor might not be evident as soon as it otherwise would, since there would be no way of knowing to which factor the 'effect' was due.

Results. A summary of the data is presented in Table III. This table shows that two sounds, one on the side of the prior stimulus and also one on the side of the stronger stimulus, are in general localized simultaneously in the middle range of the series, with a general tendency for the sound on the side of the stronger stimulus to appear at about the third point from the beginning of the series, and for the sound on the side of the prior stimulus to disappear at about the third point from the end of the series.

The first slight intensive difference is sufficient to introduce a double lateral effect in *H*'s case, and the sound on the side of the stronger stimulus remains localized very consistently at the 90° position throughout the series, while the sound on the side of the prior stimulus falls back somewhat toward the median plane and is finally blotted out near the end of the series when the intensive difference is relatively very large. Double lateral localization on the basis of the temporal factor may have been present in this case at the beginning of the series; but in *M*'s case, which is much the same as that of *H*, the double lateral effect does not appear until the fourth step in the series is reached, which means that this effect was not due to the temporal factor in this case, and also that it may not have been due to the temporal factor in *H*'s case.

In the case of *Hd*, and also *L*, the sound localized toward the side of the prior stimulus falls more or less gradually from the beginning of the series toward the median plane to a localization of approximately 15° and finally drops out altogether, while the sound on the side of the stronger stimulus does not appear until rather late in the series, at which time it is localized near the median plane, but changes fairly gradually to the maximal lateral position as the end of the series is approached.

The average deviations indicate that the localization toward the side of the prior stimulus is more difficult than the localization toward the side of the stronger stimulus.

The results therefore indicate that intensive differences not only neutralize the 'effect' of the temporal factor, but also that

such differences may either introduce a double lateral effect or condition approximately the normal 'intensity effect.'

(*4*) *Localization with temporal differences apposed with an intensive difference.* The next step in this analysis was the determination of the effectiveness of temporal differences apposed with a relatively large intensive difference. The conditions of the previous experiment were reversed, *i.e.* the stimuli were presented so that the weaker stimulus was also the prior stimulus. The stimuli were presented with an intensive difference sufficiently large to cause approximately a maximal lateral localization. In terms of the units of the intensive series, the binaural ratio was 8.32δ.

Results. The results are summarized in Table IV. A comparison of this table with Table III reveals the fact that only one sound appears when temporal differences are apposed with a relatively large intensive difference, and that this sound is always localized on the side of the stronger stimulus and fairly consistently at the maximal lateral position.

H localizes the sound at 90° position at every point in the series. *Hd* tends to localize the sound somewhat toward the median plane as the temporal difference increases, but the average

TABLE IV

LOCALIZATION WITH TEMPORAL DIFFERENCES APPOSED WITH A RELATIVELY LARGE INTENSITY-DIFFERENCE

Temporal difference	Observers							
	H		*Hd*		*L*		*M*	
σ	I	T	I	T	I	T	I	T
0.12	90±0	—	83± 3	—	87±2	—	75±22	—
0.24	90±0	—	89± 1	—	88±2	—	80±10	—
0.36	90±0	—	87± 3	—	85±6	—	80±10	—
0.48	90±0	—	81± 5	—	81±4	—	80±15	—
0.60	90±0	—	82± 6	—	83±2	—	82±11	—
0.72	90±0	—	86± 5	—	82±3	—	80±10	—
0.84	90±0	—	83± 3	—	80±4	—	70±13	—
0.96	90±0	—	82± 3	—	80±2	?	70±17	—
1.08	90±0	—	83± 4	—	79±2	?	70±22	—
1.20	90±0	—	82± 5	—	80±0	?	70±22	—
1.32	90±0	—	77±11	—	79±4	?	76±16	—
1.44	90±0	—	77±11	—	82±2	—	82± 8	—
1.56	90±0	—	76±13	—	83±2	—	86±10	—
1.68	90±0	—	75±13	—	81±3	—	75±15	—
1.80	90±0	—	73±14	—	81±5	—	74±19	—
1.92	90±0	—	73±14	—	79±7	—	74±19	—
2.04	90±0	—	72±13	—	81±6	—	84± 9	—
2.16	90±0	—	77±13	—	84±3	—	86± 6	—
3.24	90±0	—	73±12	—	85±0	—	90± 0	—
4.32	90±0	—	72±11	—	87±2	—	90± 0	?

deviations in this case also increase as the temporal interval increases. These two *O*s do not report a single case of simultaneous localization of sounds on both sides, as they do in the previous experiment.

Localization for *L* and *M* is more or less irregular, but the irregularities are so distributed that they show no general tendency toward medial localization. These *O*s sometimes thought (they were not sure and entered '?' in their record) they heard sounds on both sides, and occasionally they reported double lateral localization.

It appears, therefore, that temporal differences apposed with a relatively large intensive difference cause slight irregularities in the localization, but that such differences do not introduce a double lateral effect, do not equalize the 'effect' of the intensive difference, and do not condition localization as do intensive differences when apposed with a relatively large temporal difference.

(*5*) *Localization with temporal and intensive differences.* The last experiment of this series dealt with localization as a function of temporal and intensive differences presented together in the same direction. The purpose was to determine if these factors supplement each other in conditioning directional perception.

Results. Table V shows the results. It is evident from a comparison of this table with Tables I and II that angular displacement increases much more rapidly in the earlier stages of

TABLE V

LOCALIZATION AS A FUNCTION OF TEMPORAL AND INTENSIVE DIFFERENCES PRESENTED TOGETHER

Intensive ratio	Temporal difference	Observers			
δ	σ	*H*	*Hd*	*L*	*M*
1.00	0.00	X	X	X	X
1.13	0.12	25±36	11± 8	16± 6	4± 4
1.27	0.24	26±31	40±20	40±24	30±20
1.42	0.36	78±19	50±28	54±27	48±18
1.60	0.48	87± 5	56±20	52±18	58±24
1.80	0.60	78±18	50±15	52± 9	65±24
2.03	0.72	62±21	52±16	52± 4	38±18
2.28	0.84	42±21	60±12	54± 7	45±24
2.57	0.96	63±17	71±13	61± 7	50±24
2.87	1.08	58±19	74±11	58± 7	52±18
3.25	1.20	75± 9	85± 2	71±13	74±17
4.11	1.32	84± 5	89± 2	79± 2	84± 9
5.20	1.44	89± 1	89± 2	82± 4	88± 3
6.57	1.56	80± 0	87± 2	84± 3	90± 0
8.32	1.68	90± 0	87± 2	86± 3	90± 0
10.53	1.80	90± 0	86± 3	88± 2	90± 0

the combined series than it does when either series is presented alone. The angular displacement increases very rapidly in the first three or four steps of the series, after which there is a plateau effect over the middle range, with a sudden swing to the maximal lateral position at the end of the series. The sound comes to the maximal lateral position slightly sooner in the combined series than it does when the intensive series is presented alone, and much sooner than it does when the temporal series is presented alone.

The average deviations indicate that localization is more difficult, more indefinite and uncertain, than it is when conditioned by a single factor.

It appears, therefore, that the intensive and the temporal factors supplement each other in determining directional localization.

III. Summary and Conclusions

Localization with intensive differences is continuous and fairly uniform. The 'intensity-effect' may be described in much the same manner as is either the 'phase-effect' or the 'time-effect,' *i.e.* as corresponding more or less to a semi-circle, a 90° arc on either side of the median plane.

Intensive differences not only neutralize the 'effect' of a relatively large temporal difference; they also either introduce a double lateral effect or condition approximately the normal 'intensity-effect.'

Temporal differences cause slight irregularities in localization conditioned by a relatively large intensive difference, but such differences do not introduce a double lateral effect, do not equalize the 'effect' of the intensive difference, and do not otherwise condition localization, as do intensive differences when apposed with a relatively large temporal difference.

The temporal and the intensive factors supplement each other in their effect on localization.

More briefly, the results seem to indicate (1) that, while both time and intensity as physical variants condition localization, these factors do not function in the same manner as conditions of auditory stimulation; and (2) that intensity may be the more effective factor in determining directional perception in auditory space.

IV. Theoretical Interpretation

Since the results of this analysis indicate that intensive differences are very significant conditions in auditory directional

perception, these results may be interpreted as supporting the intensity-theory of sound localization. But whether intensity is the main determining factor throughout the entire pitch (frequency) range of auditory stimuli remains to be demonstrated.

That time-differences are not reducible to intensity-differences is fairly evident. Boring has advanced the theory that, because of inhibitory processes, the cortical excitation is greater for the prior stimulus, and that a temporal difference is therefore reduced to an intensive difference at the auditory center; *i.e.* the prior stimulus to a greater or less degree, depending on the temporal interval, inhibits the later.[20] If this theory held, it seems that temporal differences apposed with a relatively large intensive difference should be effective in the same manner as are intensive differences apposed with a relatively large temporal difference. But the results indicate that they are not. This fact fails to support the interpretation of the intensity-theory that reduces phase and time to intensity.

The results can not be considered as supporting the phase-time-intensity-theory because each factor, when the other factor was controlled, was found to condition its normal 'effect' with high-pitched sounds used as stimuli.

Since the intensive differences were found to be more effective than temporal differences in determining localization when high-pitched sounds were used as stimuli, and since these factors were found to supplement each other in conditioning localization, and, further, since intensive differences have been found to be effective, though not as effective as phase differences (defined as the time-difference in the arrival of corresponding phases at the ears), in ranges of relatively low frequency,[21] it seems that the results of the present investigation may best be interpreted in the light of the 'difference-pattern-theory.'

Intensity may be the more effective factor in determining localization under the conditions of the present investigation, but time may be the more effective factor under other conditions, while both factors may contribute, in the degree that they are present in the complex conditions of hearing, to a binaural difference-pattern that results in directional perception.

[20]E. G. Boring, *op. cit.*, 168 f.

[21]This conclusion is drawn from the investigations of Halverson and Banister, as reviewed in a previous section of this report.

THE RELATION OF HETEROGENEOUS AND HOMOGENEOUS CHROMATIC STIMULI IN THE RANGE OF VISUAL APPREHENSION EXPERIMENT

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The present paper is a continuation of the series of studies in the 'range of visual apprehension' initiated by Fernberger's study[1] in 1921, where he, as Dallenbach[2] in 1920, came to the conclusion that the classical 'range of attention' experiment is not a problem in attention but rather one in apprehension. The conscious patterns which exist in this type of experiment were first analyzed and characterised by Oberly[3] and further investigated by Cooper[4] and Hart.[5] The appropriateness of the terms used by Oberly and adopted in the succeeding papers to denote these patterns has been disputed, but the phenomena described have consistently been corroborated.[6] The recent study of Glanville and Dallenbach,[7] in addition to its verification of Oberly's analyses, contains a very complete historical account of the development of the 'range of attention' experiment and introduces a more neutral terminology than that previously employed. The terminology of the present paper conforms to that of Glanville and Dallenbach.

The previous studies from the University of Pennsylvania have already determined experimentally (1) the interrelation of the statistical limens of the three conscious patterns (Oberly), and

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[1] S. W. Fernberger, A preliminary study of the range of visible apprehension, this JOURNAL, 32, 1921, 133.

[2] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3 1920, 229.

[3] H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-352.

[4] S. F. Cooper, The effect of brightness in the range of attention experiment, *ibid.*, 40, 1928, 254-274.

[5] J. R. Hart, The range of visual attention, cognition, and apprehension for colored stimuli, *ibid.*, 40, 1928, 276-283.

[6] K. M. Dallenbach, Dr. Oberly on the 'range of visual attention, cognition, and apprehension', *ibid.*, 36, 1925, 154-156; Dr. Fernberger on the 'range of attention' experiment, *ibid.*, 38, 1927, 479-481; S. W. Fernberger, The 'range of attention' experiment, *idem.*, 478-479.

[7] A. D. Glanville and K. M. Dallenbach, Range of attention, *ibid.*, 41, 1929, 207-236.

the effect (2) of brightness (Cooper), and (3) of color (Hart) on these limens. The last was, however, concerned with only homogeneous chromatic stimuli. The present study considers both homogeneous and heterogeneous chromatic stimuli and their interrelations.

Summarily we may define, then, the problem of the present paper as an investigation of the effect of homogeneous and heterogeneous chromatic stimuli on the statistical limens of the three conscious patterns which Glanville and Dallenbach have called 'Unit,' 'Group,' and 'Mediate' Apprehension, and which correspond descriptively to those patterns designated, respectively, by Oberly as 'Attention,' 'Cognition,' and 'Apprehension.'

Experimental Procedure

The stimulus-objects used in this experiment were colored circular dots of the four primary colors on a white background. The dots, 6.5 mm. in diam., were cut from the Hering papers—saturated yellow, blue, red, and green—and were pasted on white cards, 87 mm. sq. The stimulus-cards were grouped into 7 different stimulus-series—a 'series' being a set of 12 cards, on each of which there were a different number of dots. The smallest number of dots used was 3, and the greatest number was 14. These stimulus-series were, further, classified into two different groups, a 'homogeneous,' and a 'heterogeneous' group. The homogeneous group consisted of those series the cards of which bore stimulus-dots all of a single color; the heterogeneous group consisted of those series the cards of which bore stimulus-dots of two or more colors. The homogeneous group comprised (1) series Y, in which the stimulus-dots were all yellow; (2) series B, in which the stimulus-dots were all blue; (3) series R, in which they were all red; and (4) series G, in which they were all green. The heterogeneous group comprised (1) series Y-B, any card of which contained one or more blue and one or more yellow stimulus-dots; (2) series R-G, any card of which contained one or more red and one or more green stimulus-dots; and (3) series R-G-Y-B, any card of which contained one or more stimulus-dots of each of the four primary colors.

The manner of pasting the dots on the cards was, to begin with, haphazard. That is to say, the appropriate number was placed upon each of the 12 cards of one series at random. An accidental configuration resulted in the case of each number, which was reproduced in all the series. Thus there was a 3-dot pattern, a 4-dot pattern, and so on up to a 14-dot pattern; and these patterns were repeated in all the series of both the homogeneous and the heterogeneous groups. The form-quality of the stimuli was, in this way, kept constant.

In the heterogeneous group, a question arose in regard to the distribution, on the same card, of the two or more colors. The method adopted was as follows. The colors of each of the stimulus-series R-G, Y-B, and R-G-Y-B were repeated in succession an indefinite number of times forming three definite sequences: R-G-R-G-R-G etc. in the first series; Y-B-Y-B-Y-B etc. in the

second series; and R-G-Y-B-R-G-Y-B etc. in the last series. From each of these the first three colors were taken to construct the 3-dot card in the respective series; the next 4 colors were taken to construct the 4-dot card; and so on, until the 12 cards of each of the series were constructed. In the two-color series, series R-G and Y-B, the red and green and the yellow and blue were distributed equally on the cards containing an even number of dots, and unequally distributed on the cards containing an odd number of dots. In series R-G, the 3-, 7-, and 11-dot stimulus-cards contained one more red dot than green, and the 5-, 9-, and 13-dot cards, one more green dot than red. In series Y-B, the 3-, 7-, and 11-dot cards contained one more yellow dot than blue, and the 5-, 9-, and 13-dot cards, one more blue than yellow dot. In series Y-B, the yellow dots were given the position of the red dots, and the blue dots that of the green dots, of series R-G. This procedure was adopted so that there would be a greater evenness of color distribution in the series as a whole, the yellow and green pigment papers having a higher luminosity than the blue and red. In the four-color series, the colors were distributed equally only on the 12-dot stimulus-card, there being on this card 3 dots of red, three of green, three of yellow, and three of blue. As far as possible the different colors were distributed on the cards of this series so that the same color did not occupy contiguous positions.

The 7 series of 12 cards each were exposed in rotation by means of a Whipple tachistoscope,[8] the exposure time being the same as Oberly's,[9] 37.5 σ calibrated by the tuning-fork method. The order in which these 84 cards were exposed was haphazard, but no cards containing dots of the same color or color combination, or containing the same number of dots were exposed successively, except that 6-dot cards followed each other in one instance, and 7-dot cards followed each other in another instance. The position in which the cards were placed in the tachistoscope for the first exposure was recorded for each card. For the second exposure, the cards were rotated from this position 90°; for the third exposure, 180°; and for the fourth exposure, 270°. This manner of rotating the cards was followed throughout the experiment. For the fifth exposure, the cards were again in the first position. By following this method, the same card could appear in exactly the same position only after 336 exposures. In this way, and by terminating each period of observation at different places in the series so that a different card was exposed first at successive periods, the *O* was prevented from learning the order of exposure and from readily recognizing any particular card.

The experiments were conducted in a dark-room. The only light constantly present was that of the exposure lamps, thus keeping constant the illumination of the stimulus cards and the general illumination. *O* sat 1.9 m. from the exposure screen. He was instructed to look at the fixation-point on this screen at the signal word 'Ready.' A second later, the experimenter released the weighted arm of the tachistoscope, simultaneously giving the second signal word 'Now,' and the stimulus-card was exposed automatically. *O* then gave his report verbally, as quickly as possible, according to instructions which will

[8] G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1924, 264 ff.
[9] Oberly, *op. cit.*, 334.

be described below, and *E* immediately recorded it. The *Os* were given practice in following these instructions before any judgment was recorded, and they were allowed to look at the instructions at the beginning of successive periods of observation whenever they so desired.

Observers. We used 5 *Os*—*A*, *G*, *M*, *N*, and *R*, all with scientific training. None of them had acted as *Os* in the earlier experiments mentioned above, but *A* and *G* were familiar with the type of work and the general experimental procedure. All the *Os* had, however, sufficient laboratory practice to put them at ease in the experimental situation of this problem and to enable them to accommodate quickly and adequately to the special procedures that we used.[10]

Instructions. The following instructions, essentially the same as those used by Oberly,[11] were given the *Os*.

"You will be shown successively, for a very short exposure, cards which contain a varying number of dots on a white background. Immediately after the exposure you will report verbally (1) the number of dots you have seen; (2) the degree of assurance of certainty of your judgment, using the following scale for your report: 5—if it is a 100 to 1 bet, 4—if it is a 5 to 1 bet, 3—if it is a 1 to 1 bet, 2—if it is a 1 to 5 bet, and 1—if it is a guess or if you do not know; (3) the method of determining your report—whether by 'immediate' judgment (*i.e.* ungrouped), by 'grouping,' or by 'counting.' 'Immediate' means that the dots are perceived in a single flash of attention and were ungrouped. 'Grouping' means that the dots are seen in a single flash of attention but were grouped. 'Counting' means that the report is made on the basis of some re-imaging of the stimulus-material, *i.e.* some mediating conscious process is used and the dots are counted in one way or another."

Two of the *Os* (*N* and *R*) returned 50 judgments for every one of the 84 stimulus-cards, and the other 3 (*A*, *G*, and *M*) returned 100 judgments for every card. The different series of cards were exposed on an average from 3 to 4 times in an observational hour. During the early stages of the experiment not so many exposures were made, but during the later stages more exposures were possible because the 3-dot cards were dropped when the *Os*, due to practice, invariably reported 4- and 5-dot cards correctly, and the 4-dot cards of the homogeneous series (except the yellow dot card) were dropped when the *Os* invariably reported 5- and 6-dot cards correctly. In the cases of 2 of the *Os* (*M* and *R*) all but three of the 5- and 6-dot cards (those of Series Y, Y-B, and R-G-Y-B) were dropped, late in the experiment, when the 7- and 8-dot cards were invariably reported correctly.

The average length of the observational periods was one hour, and the *Os* usually observed three periods a week. The *Os* were permitted frequent rests. In the cases of the *Os* who returned 100 judgments for every stimulus-card,

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[10]I wish to express my appreciation of the splendid coöperation of my *Os*, and for their industry throughout the experimental period. My thanks are also due Professor Fernberger who suggested the problem and under whose direction it was carried out. His advice and criticism were both a help and an encouragement.

[11]Oberly, *op. cit.*, 335. Cf. also S. F. Cooper, *op. cit.*, 257; Hart, *op. cit.*, 276.

an interval of at least a week lapsed between the first and second series of 50 judgments. The experimental work was begun in the winter of 1927 and completed in the summer of 1928.

Results

In the following tables, the data based upon the judgments characterized as 'immediate' by the *O*s are classified under 'unit' apprehension; these data plus those based on the judgments characterized as 'grouped' are classified under 'group' apprehension; and these two types plus the data based on judgments characterized by the *O*s as 'counted' are classified under 'mediate' apprehension.[12] Judgments that were given with an assurance of less than '3,' or that did not give the correct number of dots exposed, were not considered in the statistical treatment of the results.

The observed relative frequencies. An examination of the percentages of correct judgments with an assurance of '5,' '4,' or '3' shows that the number of dots apprehended increases from 'unit' apprehension to 'group' and to 'mediate.'

A comparison of the results of the first and second series of 50 judgments—for those *O*s who returned 100 judgments for every stimulus-card—shows that the percentages correctly reported are greater for the second than for the first set. The increase is especially noticeable in 'group' and 'mediate' apprehension, particularly in the data of *G* and *M*. It is conspicuously apparent in the reports on stimulus-cards containing from 3 to 5 dots.

The limens. The statistical limens and measures of precision shown in Table I were obtained from the data by the method of constant stimuli. The limens of every stimulus-series increase, as a rule, from 'unit', to 'group', to 'mediate' apprehension.

The limens derived from the second series of 50 judgments show the effect of practice. They are greater in magnitude than those derived from the first, and the increase is largest for 'mediate' apprehension, less large for 'group' apprehension, and smallest for 'unit' apprehension.

[12]For an explanation and justification of this terminology see K. M. Dallenbach, Dr. Oberly on 'the range for visual attention, cognition and apprehension', this Journal, 36, 1925, 155; and Glanville and Dallenbach, *op. cit.*, 220 f.

TABLE I

SHOWING THE LIMENS (L) AND THE MEASURES OF PRECISION (h) FOR EVERY ONE OF THE STIMULUS-SERIES FOR EVERY COGNITIVE STATE

O	Stimulus-series	'Unit' apprehension L	'Unit' apprehension h	'Group' apprehension L	'Group' apprehension h	'Mediate' apprehension L	'Mediate' apprehension h
	Y	6.2	0.62	7.4	0.38	7.5	0.35
	B	6.2	1.02	7.3	0.33	7.4	0.31
	R	6.6	1.18	7.1	0.32	7.2	0.33
A	G	6.3	1.11	7.6	0.27	7.7	0.29
	R-G	6.8	0.44	7.1	0.38	7.1	0.34
	Y-B	5.1	0.51	7.2	0.27	7.3	0.40
	R-G-Y-B	6.4	0.78	7.2	0.36	7.2	0.23
	Y	5.8	0.37	6.1	0.32	6.7	0.24
	B	5.5	2.56	7.4	0.69	8.5	0.66
	R	5.1	2.11	7.3	0.74	8.6	0.61
G	G	5.7	4.23	7.4	0.49	8.5	0.27
	R-G	5.3	1.44	7.0	0.54	8.5	0.48
	Y-B	4.2	0.69	5.0	0.38	5.4	0.33
	R-G-Y-B	5.1	0.43	5.4	0.40	5.8	0.32
	Y	7.3	0.15	7.8	0.23	8.1	0.25
	B	8.8	0.37	8.1	0.47	9.7	0.26
	R	7.9	0.53	8.3	0.62	9.6	0.30
M	G	8.2	0.50	7.9	0.64	9.8	0.26
	R-G	8.2	0.08	8.2	0.08	9.8	0.26
	Y-B	6.7	0.56	6.8	0.48	8.4	0.28
	R-G-Y-B	6.8	0.49	6.9	0.47	9.4	0.27
	Y	5.3	0.47	6.1	0.35	7.0	0.28
	B	6.6	1.03	7.2	0.77	9.1	0.35
	R	6.8	1.71	7.3	1.04	9.0	0.29
N	G	6.7	1.29	7.5	0.76	9.1	0.35
	R-G	6.5	0.66	6.3	0.47	8.4	0.32
	Y-B	6.2	1.02	6.2	0.58	7.1	0.31
	R-G-Y-B	6.2	0.66	6.6	0.59	7.9	0.37
	Y	6.0	0.43	7.7	0.30	10.1	0.21
	B	7.2	0.46	8.6	0.33	10.4	0.1_1
	R	7.0	0.51	8.8	0.33	12.0	0.1_8
R	G	6.8	0.46	9.0	0.30	11.8	0.2_0
	R-G	6.6	0.47	8.4	0.37	10.2	0.1_9
	Y-B	6.2	0.70	6.8	0.33	8.7	0.1_5
	R-G-Y-B	6.7	0.53	8.0	0.30	10.2	0.2_0

The rank orders of the different stimulus-series. Table II, derived from Table I, shows the rank orders of the limens of the different stimulus-series based upon the first set of 50 judgments. Rank 1, or first place, was given to the series whose limen was greatest in magnitude; rank 2, or second place, was given to the series with the next greatest limen; and so on to rank 7, or last place, which was given to the series whose limen was smallest in magnitude.

The data given in Table II were cast in the form of the scattergrams (Table III) which in turn were used as the basis of the final classification of the stimulus-series shown in Table IV.

The rank order given in Table IV was derived from Table III as follows: for every stimulus-series the sum of its rank orders 1–4 was compared with that of its rank orders 4–7. If the former was greater it was placed in the 'high' group; if the latter was greater it was placed in the 'low' group; and if the sums were approximately equal it was placed in the 'medium' group. It will be observed that the final order for the different stimulus-series is, with one exception, the same for every cognitive pattern. The exception occurs in 'unit' apprehension in which the stimulus-series occupying ranks 5 and 6 are the reverse of the ranks occupied in the other two cognitive patterns.

TABLE II

THE RANK ORDER OF THE STIMULUS-SERIES BASED ON THE MAGNITUDE OF THEIR RESPECTIVE LIMENS FOR EVERY *O* AND FOR EVERY COGNITIVE PATTERN

Cognitive pattern	Rank order	Observers				
		A	*G*	*M*	*N*	*R*
'Unit' apprehension	1	R-G	Y	B	R	B
	2	R	G	R-G	G	R
	3	R-G-Y-B	B	G	B	G
	4	G	R-G	R	R-G	R-G-Y-B
	5	B	R-G-Y-B	Y	R-G-Y-B	R-G
	6	Y	R	R-G-Y-B	Y-B	Y-B
	7	Y-B	Y-B	Y-B	Y	Y
'Group' apprehension	1	G	G	R	G	G
	2	Y	B	R-G	R	R
	3	B	R	B	B	B
	4	R-G	R-G	G	R-G-Y-B	R-G
	5	R-G-Y-B	Y	Y	R-G	R-G-Y-B
	6	R	R-G-Y-B	R-G-Y-B	Y-B	Y
	7	Y-B	Y-B	Y-B	Y	Y-B
'Mediate' apprehension	1	G	R	G	B	R
	2	Y	G	R-G	G	G
	3	B	B	B	R	B
	4	Y-B	R-G	R	R-G	R-G
	5	R-G-Y-B	Y	R-G-Y-B	R-G-Y-B	R-G-Y-B
	6	R	R-G-Y-B	Y-B	Y-B	Y
	7	R-G	Y-B	Y	Y	Y-B

The stimulus-series were similarly classified into 'high,' 'medium,' and 'low' upon the basis of the average limens of all the *O*s. These data appear in Table V. The rank orders given in Table IV may be called the 'preferential' orders, while those given in Table V may be called the 'statistical' order. The orders in both instances are, however, substantially the same; only five points of disagreement occur—two in 'unit' apprehension, one in 'group' apprehension, and two in 'mediate' apprehension—and they consist of the reversal of neighboring rank orders.

The classification of the stimulus-series into 'high,' 'medium,' and 'low' is, with one exception, the same in the two tables. The exception occurs in 'unit' apprehension in which the rank orders of series R and R-G are reversed. The numerical difference between these two series is, however, so small—0.024—that the discrepancy may be said not to affect the general tendency shown by

these tables. The highest limens—in order of preference—in all of the cognitive patterns for all of the *Os* collectively are stimulus-series G, B, and R; the medium limen is R-G; and the lowest limens are Y, R-G-Y-B, and Y-B.

TABLE III

SCATTERGRAMS OF THE RANK ORDER OF EVERY ONE OF THE SEVEN STIMULUS-SERIES FOR EVERY COGNITIVE PATTERN

Cognitive pattern	Rank order	Stimulus-Series						
		Y	B	R	G	R-G	Y-B	R-G-Y-B
'Unit' apprehension	1	1	2	1	0	1	0	0
	2	0	0	2	2	1	0	0
	3	0	2	0	2	0	0	1
	4	0	0	1	1	2	0	1
	5	1	1	0	0	1	0	2
	6	1	0	1	0	0	2	1
	7	2	0	0	0	0	3	0
'Group' apprehension	1	0	0	1	4	0	0	0
	2	1	1	2	0	1	0	0
	3	0	4	1	0	0	0	0
	4	0	0	0	1	3	0	1
	5	2	0	0	0	1	0	2
	6	1	0	1	0	0	1	2
	7	1	0	0	0	0	4	0
'Mediate' apprehension	1	0	1	2	2	0	0	0
	2	1	0	0	3	1	0	0
	3	0	4	1	0	0	0	0
	4	0	0	1	0	3	1	0
	5	1	0	0	0	0	0	4
	6	1	0	1	0	0	2	1
	7	2	0	0	0	1	2	0

The relation of heterogeneous to homogeneous stimuli. The distribution of the stimulus-series in Tables IV and V indicate the relative influence of homogeneous and heterogeneous stimuli on the *Os*' limens as a group. In every one of the cognitive patterns three of the four homogeneous stimulus-series make up the group of 'high' limens, and two of the three heterogeneous series fall within the group of the 'low' limens. Series Y, a homogeneous series, falls within the 'low' ranks, holding rank 6 in 'unit' apprehension, and rank 5 in 'group' and 'mediate' apprehension. Series R–G holds the fourth or 'medium' rank in all three of the cognitive patterns. From these data we may conclude that, under the conditions of our experiment, heterogeneity of stimuli gives, in most cases, a smaller limen than homogeneity. It is to be observed, however, that Y is a common factor in each member of the 'low' limen group, and, as we have seen, stimulus-series Y falls always within the 'low' order for all the cognitive patterns.

TABLE IV

RANK ORDER OF THE STIMULUS-SERIES FOR THE *Os* AS A WHOLE FOR EVERY COGNITIVE PATTERN, CLASSIFIED INTO GROUPS DESIGNATED 'HIGH,' 'MEDIUM,' AND 'LOW.' (FROM TABLE III)

Rank order		Cognitive patterns		
		'Unit' apprehension	'Group' apprehension	'Mediate' apprehension
	1	G	G	G
High	2	B	B	B
	3	R	R	R
Medium	4	R-G	R-G	R-G
	5	R-G-Y-B	Y	Y
Low	6	Y	R-G-Y-B	R-G-Y-B
	7	Y-B	Y-B	Y-B

TABLE V

RANK ORDER OF THE STIMULUS-SERIES FOR EVERY COGNITIVE PATTERN, BASED ON THE AVERAGE OF THE LIMENS OF ALL THE *Os* (From Table I)

Rank order		Cognitive patterns					
		'Unit' apprehension		'Group' apprehension		'Mediate' apprehension	
		Series	Av. L	Series	Av. L	Series	Av. L
	1	B	6.9	G	7.9	G	9.7
High	2	G	6.7	R	7.8	R	9.3
	3	R-G	6.7	B	7.7	B	9.0
Medium	4	R	6.9	R-G	7.4	R-G	8.8
	5	R-G-Y-B	6.3	Y	7.2	R-G-Y-B	8.1
Low	6	Y	6.1	R-G-Y-B	6.8	Y	7.9
	7	Y-B	5.9	Y-B	6.4	Y-B	7.4

It is also to be observed that the homogeneous and the heterogeneous stimulus-series holding the first four positions in the rank order for the *Os* as a whole are characterised by the absence of Y. From these facts, we may conclude that Y is an indispensable part of the cause of the low rank order of the heterogeneous stimuli. An experimental determination of this conclusion could be made by running additional two-color series, Y-R and Y-G, and three-color series, Y-R-G, Y-B-G, and Y-B-R.

The relation of the heterogeneous and homogeneous stimuli, in the case of every *O*, is shown quantitatively by Table VI. The statistical limens of each of the *Os* for the four homogeneous series and for the three heterogeneous series in each of the cognitive patterns were averaged, and the difference between these averages was taken. In every instance, the average heterogeneous limen is smaller than the homogeneous limen. If a dif-

ference of 0.5 or more is taken to be significant, there is a significant difference in 9 of the 15 cases (60%), and this difference increases from 'unit' to 'group,' to 'mediate' apprehension. For one *O*, *G*, there is a significant difference in each of the three cognitive patterns; for two *O*s, *R* and *N*, there is a significant difference in 'group' and 'mediate' apprehension; and for *M* there is a significant difference in 'unit' and 'group' apprehension. For *A* the difference is not significant in any of the cognitive patterns. We conclude from these data, therefore, that the range of visual apprehension for heterogeneous chromatic stimuli compared with that for homogeneous chromatic stimuli is significantly reduced.

This narrowing of the range of apprehension for heterogeneous stimuli is caused, in part, by heterogeneity, but the presence of Y is, we found, also part of the cause. Table VI, however, does not show to what extent Y is a causal factor, because Y appears in both the homogeneous and the heterogeneous groups and, therefore, affects both the homogeneous and the heterogeneous average limens. In order to show specifically the effect of Y, Table VII was constructed. The numbers, in Table VII, designated as 'high' are the averages of the three highest limens, those of stimulus-series G, B, and R (Table IV), and those designated as 'low,' the averages of the lowest limens, those of series Y, R-G-Y-B, and Y-B; series R-G was omitted from these calculations because

TABLE VI

THE AVERAGE LIMENS OF THE HOMOGENEOUS AND OF THE HETEROGENEOUS STIMULUS-SERIES AND THEIR DIFFERENCES FOR EVERY *O* FOR EVERY ONE OF THE COGNITIVE PATTERNS

O	'Unit' apprehension			'Group' apprehension			'Mediate' apprehension		
	Homo.	Hetero.	Dif.	Homo.	Hetero.	Dif.	Homo.	Hetero.	Dif.
A	6.3	6.1	0.2	7.4	7.2	0.2	7.4	7.2	0.2
G	5.5	4.9	0.6	7.1	5.8	1.3	8.1	6.2	1.9
M	8.0	7.3	0.7	8.0	7.3	0.7	9.4	9.2	0.2
N	6.4	6.3	0.1	7.2	6.3	0.7	8.5	7.8	0.7
R	6.8	6.5	0.3	8.5	7.7	0.8	11.0	9.7	1.3

TABLE VII

THE AVERAGE LIMENS OF THE 'HIGH' AND OF THE 'LOW' STIMULUS-SERIES AND THEIR DIFFERENCES FOR EVERY *O* FOR EVERY ONE OF THE COGNITIVE PATTERNS

O	'Unit' apprehension			'Group' apprehension			'Mediate' apprehension		
	High	Low	Dif.	High	Low	Dif.	High	Low	Dif.
A	6.3	5.9	0.4	7.4	7.3	0.1	7.4	7.3	0.1
G	5.4	5.1	0.3	7.4	5.5	1.9	8.5	5.9	2.6
M	8.3	6.9	1.4	8.1	7.1	1.0	9.9	8.6	1.3
N	6.7	5.9	0.8	7.3	6.3	1.0	9.0	7.3	1.7
R	7.0	6.3	0.7	8.8	7.5	0.3	11.4	9.6	1.8

it gave the medium limen throughout. By this method Y was eliminated entirely from the set of averages designated as 'high,' and was a factor in every one of the series whose limens were averaged for the set designated as 'low.' In this way, then, the influence of Y on the thresholds may be more specifically shown.

The difference, for the majority of *O*s, between the 'high' and the 'low' average limens (Table VII) is greater than that between the homogeneous and heterogeneous average limens (Table VI)—the difference in the former case being less than 0.5 only four times, while in the latter case it is less six times, out of fifteen. In other words, the effect of Y is significant in 73.6% of the instances, while the effect of heterogeneity was, we saw, significant in only 60% of the instances. At least one significant difference is to be noted in Table VII for every *O*: *M* and *N* show significant differences in all three of the cognitive patterns; *R* and *G* in two of the patterns; and *A* in one. It is obvious from these data, then, that for our group of *O*s, as a whole and under our experimental conditions, Y reduces the range of visual apprehension for chromatic stimuli more, and to a greater extent, than does heterogeneity of stimuli.

Table VIII gives quantitative corroboration to the general conclusions that followed from the data given in Tables IV and V, namely, that the R-G limens differ least from the three high homogeneous limens, the Y-B limens differ most from the homogeneous limens and the other heterogeneous limens, and the Y limens differ very little from the low heterogeneous limens. If we consider the average differences, tabulated in the last column of Table VIII, for each cognitive pattern, we observe, in the first place, that the average limens for R-G do not differ significantly from the average of all the homogeneous limens, the R-G average limen being only 0.105 larger in 'unit' apprehension, 0.199 smaller in 'group' apprehension, and 0.070 smaller in 'mediate' apprehension, than the average homogeneous limen. In the second place, the average limen for Y-B is significantly smaller in each cognitive pattern than the average limens of the homogeneous series as a whole, the Y-B limen being 0.900 smaller in 'unit,' 1.212 smaller in 'group,' and 1.499 smaller in 'mediate' apprehension than the average homogeneous limen. The average limen for R-G-Y-B is also significantly smaller than the average limens for the homogeneous stimulus-series as a whole in 'group' and 'mediate' apprehension, but not in 'unit.' In the third place, the average limen for stimulus-series Y is not significantly different from the average limens for the heterogeneous stimulus-series in any of the cognitive patterns: the limen for Y being but 0.092 larger in 'unit' apprehension, 0.128 smaller in 'group' apprehension, and 0.069 larger in 'mediate' apprehension.

The averages tabulated in the bottom row of Table VIII show the amount and the direction of the differences of the averages for the heterogeneous and the homogeneous limens for all the *O*s. The average heterogeneous limens differ significantly in every cognitive pattern from the average homogeneous limens with the exception of the case of stimulus-series Y. In 'unit' apprehension the average heterogeneous limen differs most from the average limen for stimulus-series B; in 'group' and 'mediate' apprehension it differs most from

the average limens for series G. In general, the average differences when minus, *i.e.* when the heterogeneous limens are smaller than the homogeneous, increase from 'unit,' to 'group,' to 'mediate' apprehension.

TABLE VIII

THE AVERAGE OF THE DIFFERENCES OF EACH OF THE HETEROGENEOUS LIMENS AND EACH OF THE HOMOGENEOUS LIMENS FOR EACH COGNITIVE PATTERN FOR THE *Os* AS A WHOLE

(The plus and minus signs indicate that the average heterogeneous limen is larger or smaller, respectively, than the average homogeneous limen compared with it)

Cognitive pattern	Hetero-geneous	Homogeneous				
		Y	B	R	G	Av.
'Unit' apprehension	R-G	+0.585	−0.168	+0.024	−0.023	+0.105
	Y-B	−0.437	−1.191	−0.998	−0.975	−0.900
	R-G-Y-B	+0.129	−0.623	−0.430	−0.407	−0.283
	Av.	+0.092	−0.661	−0.468	−0.468	
'Group' apprehension	R-G	+0.412	−0.364	−0.337	−0.506	−0.199
	Y-B	−0.596	−1.353	−1.365	−1.534	−1.212
	R-G-Y-B	−0.200	−0.937	−0.949	−1.118	−0.776
	Av.	−0.128	−0.885	−0.884	−1.053	
'Mediate' apprehension	R-G	+0.930	−0.016	−0.485	−0.711	−0.070
	Y-B	−0.478	−1.644	−1.893	−1.982	−1.499
	R-G-Y-B	−0.246	−0.900	−1.169	−1.259	−0.894
	Av.	+0.069	−0.853	−1.182	−1.317	

The conscious patterns. The *Os* were required by the instructions in this experiment to designate the method of determining their reports as 'immediate,' 'grouped,' or 'counted,' according to certain general characterizations given them. This was not as simple as we had been led by previous experiments to believe. All the *Os* experienced difficulty at times in deciding the method which served as the basis of their reports.

The first category, 'immediate,' appeared to be unequivocal. Whenever the number of dots was apprehended immediately in a single flash of consciousness the judgment was given promptly and without hesitation. A few times, however, *G* and *N* reported some difficulty in deciding whether the judgment was 'grouped' or 'immediate.'

The second category, 'grouped,' appeared to be equivocal. This term was, by definition, used to designate the conscious pattern

when the stimulus was perceived immediately, the number of dots appearing with different degrees of clearness, that is, attentional grouping of the dots occurred. This method of determining the report occurred rarely and it was thought by most of the *O*s to be due to some factor in the stimulus, *e.g.* to the different colors, to the particular arrangement of the dots on the stimulus-cards, or to both.

M reported that the conscious pattern 'grouped' was rarely experienced. Of the recorded judgments of this *O*, only 16 of the 4200 judgments of the first set were characterized as 'grouped,' all of these being early in the experimental work; none of the second set of judgments was so characterized.

G reported that he "was sometimes in doubt whether the judgment was 'immediate' or 'grouped,' " and found it "difficult to know if the grouping was objectively given or did I do it myself." Most of the judgments of the 6-dot stimulus-cards were given as 'grouped' by *G* but he felt that this grouping was certainly due to the objective arrangement of the dots.

N, who gave a fair percentage of 'grouped' judgments, stated that "grouping by colors was the rule." He commented that "anything above a 5, that is, the 6s and 7s, usually were grouped."

A reported that "grouping depends on the position of dots on the cards." A judgment was called 'grouped' when the dots were "arranged in a number of separate groups." *A* gave the highest percentage of 'grouped' judgments; the psychometric functions of 'group' and 'mediate' apprehension, graphically presented, appear in his case almost to coincide.

The third category, 'counted,' appeared to be not as unequivocal as the first but less equivocal than the second. A report made on the basis of some subsequent, mediating, mental processes, involving some kind of re-imaging of the stimulus material, was called 'counted.' Most of the *O*s characterized the greater part of the judgments recorded in the third category, "grouped plus counting." They reported that the dots were divided into groups, wholly or in part, and the final sum was found by adding together the sum of the individual groups or, by adding together the sum of the groups and the remaining single scattered dots.

The introspections raise a question as to the causes, under the conditions of this experiment, of the variability of the categories in their application. Our experimental data do not, however, give us the material for an answer. Further experimental work is needed to determine what, under the conditions of our experiment, are the causes affecting the stability of these categories.

The effect of color on the range of visual apprehension. Hart investigated the effect of chromatic homogeneous stimuli on the range of visual apprehen-

sion and found that red gave the largest limen, green the smallest, and yellow and blue limens of intermediate size.[12] We found in this experiment that in general green gave the largest limen, yellow the smallest, and blue and red limens intermediate in size. In other words, the order of single colors based on the serially decreasing magnitude of the respective limens was found by Hart to be (1) red, (2) yellow, (3) blue, (4) green; while the order on the basis of our data was found to be (1) green, (2) blue, (3) red, (4) yellow. How may these differences be accounted for? The same set of chromatic papers were used in each experiment, and the stimulus-objects were dots. Hart, however, presented the dots on a gray background, while in the present study the dots were presented on a white background, the other conditions of the two experiments being the same—the pre-exposure field being black and the tachistoscope being operated in a dark-room. The difference in the background of the stimulus dots may or may not be the chief cause of the differences in the magnitude of the limens of the respective colors in the two instances, but, in either case, it must be considered part of the cause of the recorded differences.

According to the introspective notes, the *O*s in our experiment experienced difficulty in making judgments at certain times, if not all the time, on account of the characteristics of a single color or of the grouping of different colors, although each *O* remarked, at some time during the experiment, "color doesn't seem to make much difference."

In the case of single colors, yellow occasioned the greatest difficulty. This was due, it was reported by every *O* at some time, to the "faintness of the yellow," which made it "difficult to see the yellow." At the beginning of the experiment, when a card of stimulus-series Y was exposed, the *O*s reported "only a flash of yellow," or "nothing at all," or "just a bunch of yellow."

M reported at first exposure of the 8-dot card of series Y, "can't see anything at all;" and at the 37th exposure of the cards, "yellow always hard to get."

R remarked on the 7th exposure of the 4-dot card of series Y, "white hardest to see," on the 14th exposure of the 8-dot card, series Y, "can't see at all," and on the 17th exposure, 13-dot card, series Y, "can't see all the yellow dots."

N stated, "I do most of my counting from a kind of after-image and yellow doesn't leave much of a memory image." At the 1st, 17th, 20th, and 42d exposures of some of the cards of series Y, he reported, "yellow, but I can't see the number, too faint."

For *G*, from the 1st to the 50th exposure, in both the first and second sets of judgments, the yellow dots were always a cause for some exclamation of disgust or displeasure, especially when more than 6 dots were exposed.

In the case of mixed colors, fewer difficulties were experienced but more were occasioned by Y than by the other single colors, R, G, and B. When cards of 8 dots and upwards of the four-color series R-G-Y-B were exposed several of the *O*s spoke of the "awful mixtures." The majority of the *O*s re-

[12] J. R. Hart, *op. cit.*, 283.

ported that the Y-B combination was the hardest, but it was the Y-factor that was mentioned as the most bothersome, here again, due to its faintness.

Thus, we may say that Y appears to have had, introspectively, more effect on the judgments of the number of stimulus dots apprehended than any other single color and any color combination.[14]

The dot-patterns and their configuration. A phenomenon occurred during the progress of the experiment which we shall call, for want of a better name, 'configuration of the dot-patterns.' By this term, we mean, the tendency on the part of the *O*s to perceive a certain number of stimulus dots as in a particular form or configuration and, then, consciously or unconsciously, to take hold of the particular configuration perceived as the 'sign' of the number of dots exposed.

For example, *A* reports, "after several trials a number of the arrangements of dots was nearly always the same and I could be sure of five even though I had not been able to perceive all the dots, just because of the arrangement, for six never appeared in certain of the arrangements which five always appeared in. With the larger number of dots, when a certain arrangement appeared time after time, I learned to give attention more definitely to a certain part of the dots, and as a consequence could even increase the number of dots which I could perceive."

G reported, "as the experiment goes on apperception plays an increasingly greater rôle, sometimes consciously, sometimes unconsciously. I've observed that I have been giving degrees of assurance in the latter part of the experiment that I couldn't have given before, take 7 for example, no matter how turned around, I always get it in groups of 3 and 4. I got to know the pattern; and if I didn't see all of the dots, due to lax attention, I'd fill it in. The manner of judging goes toward the 'immediate'-end by getting less 'grouped' and 'counted' and more just 'grouped.' Now I know how the patterns of 6 and 7 look. . . . and 10 equals three lines and a curly cue. I don't have to give my full attention, just a glimpse is sufficient."

At the beginning of the experimental work, however, the cards of the various series containing the same number of dots on different exposures appeared, to all the *O*s, to be different. *G*, for example, remarked that "even when there are the same number of dots, the pattern seems different and therefore it is sometimes easier and sometimes harder to judge the number;" and suggested that "the same number of dots in each series might have been arranged in the same pattern." It was not until later in the experiment that the *O*s perceived certain numbers as particular configurations.

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[14]The term configuration as used in this paper does not imply any opinion, on the part of the writer, as to the positions taken by the *Gestalt* School whose fundamental concept, *Gestalt* or *Struktur*, has, in English, been rendered by the term 'configuration.'

Spontaneous comments on the special configurations of certain numbers of dots were made earlier or later in the experimental work and at different times by the different *O*s. *M*, on the 8th exposure of the stimulus cards observed that a 7-dot card was "upside down," and on the 22d exposure greeted a 7-dot series B card as an "old friend," apprehending the number of dots 'immediately' (previously the number had been reported 'counted'). *N* on the 28th exposure stated that the 7- and 6-dot cards were "remembered." *R* on the 41st exposure remarked, after a 7-dot card had been exposed, "a bunch of sevens of this form." *G* was not sure of the 7-dot pattern in any of the series until the second set of 4200 judgments, that is, after, at least, 50 exposures. *A* made no specific comment on the 7-dot pattern in either the first or second set of judgments.

Such variability from *O* to *O* in the time of the beginning of configuration of the dots of the different stimulus-cards, as we have indicated in the case of 7 dots, was shown in the case of all the numbers of stimulus dots (that were configurated) above 5 or 6, and varied from series to series for any one *O*. Further, configuration seemed to reach an upper limit at different numbers of dots for different series, varying among the *O*s, the limit probably bearing a direct relation to the respective limens for 'unit' apprehension.

The factors upon which this configuration depends are not specifically given among the experimental data of the present investigation. Certain indications are, however, apparent. It may be held that configuration depends, in this experiment, upon the stimuli, since the dot-patterns were the same for all the series. But the manner of rotating the stimulus cards for each successive exposure, so that not before 336 exposures were made could the same card appear again, and the fact that cards with the same dot-pattern were not, at any time, exposed consecutively prevented the *O*s from easily learning the order of the exposure and readily recognizing the dot-patterns.

It is, no doubt, true that configuration, in our experiment, depends, in part, upon certain objective factors, for example, the arrangement of the dots upon the stimulus cards and the color or color-mixture of the dots. But it is also true that configuration depends, in part, upon certain subjective or psychosomatic factors, for our experimental data show that configuration began at different times in the progress of the experiment in the case of different *O*s, and that the extent and degree to which this phenomenon affected the judgments varied from *O* to *O*. It appears, therefore, that configuration depends, in part, upon the peculiar mental abilities and 'set,' of the individual *O*s. Thus, we may say that configuration is both a function of the stimulus and of the experiencing individual.

Conclusions

(*1*) The statistical limens for each of the three cognitive patterns for all the stimulus-series hold, in general, the following relations: the value of the limens for 'unit' apprehension are smallest in magnitude; those for 'mediate' apprehension, largest; and, for the most part, those for 'group' apprehension are intermediate.

(*2*) The interrelation of the statistical limens of the different series for each of the three cognitive patterns is, in general, as follows: the limens greatest in magnitude are given, on the average, by series G, B, and R; the limens intermediate in magnitude, by series R-G; and the limens smallest in magnitude, by series Y, R-G-Y-B, and Y-B.

(*3*) The color preference of the group of *O*s as a whole, indicated by the serially decreasing magnitude of the respective limens, is as follows: (1) G, (2) B, (3) R, (4) R-G, (5) Y, (6), R-G-Y-B, (7) Y-B. The order of the homogeneous series according to the decreasing magnitude of respective limens is (1) G, (2) B, (3) R, and (4) Y; and of the heterogeneous series, (1) R-G, (2) R-G-Y-B, and (3) Y-B.

(*4*) Heterogeneity of chromatic stimuli tends, under the conditions of this experiment, to reduce the magnitude of the limens for each of the three cognitive patterns, least for 'unit' and most for 'mediate' apprehension.

(*5*) The results of the present study indicate that yellow is an indisputable factor in the cause of this reduction of the range of visual apprehension by heterogeneity of chromatic stimuli, but what the exact rôle of yellow is cannot be determined under the conditions of the experiment and would require a special investigation.

(*6*) A comparison of the introspective notes of our *O*s indicated a certain degree of instability in the functioning of the three categories under the conditions of our experiment. The category 'immediate' appeared to be least equivocal, that of 'counted,' medially so, and that of 'grouped,' most equivocal.

(*7*) As the experiment progressed, and perhaps as the effect of practice, the stimulus-dots became configurated. The *O*s

recognized the dots as having a definite form or pattern. This configuration affected the *O*s' judgments in two respects: (1) conscious momentary "wanderings of the attention" did not interfere with the apprehension of the correct number of dots; the recognition of their structure, sometimes accomplished in the periphery of consciousness, acted as a 'sign' of the number of dots; (2) the conscious pattern 'counted' disappeared in most cases after the configuration of certain stimulus-cards was established, and in these cases the conscious patterns 'immediate' or 'grouped' appeared instead.

The amount of practice required before this configuration began varied from *O* to *O*, and in the case of every *O* the number of dots which formed such a figure, within the limits set by this experiment, was relative to the statistical limen for the stimulus-series in question.

The specific factors upon which this configuration depends cannot be determined without further experimentation, but it appears to be both a function of the stimulus and of the experiencing individual.

EYE-MOVEMENTS AND THE PHI-PHENOMENON

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One of the most common explanations advanced in the history of psychology for different kinds of visual phenomena has been the eye-movement hypothesis. It has been advanced to explain the perceptions of forms and symmetry, the visual illusions, the autokinetic sensation or Charpentier's illusion, illusions of reversible perspective, and many other facts. It has been revived lately as a possible explanation for a number of the phenomena observed in the perception of movement from stationary stimuli, or, as it has come to be known, the phi-phenomenon. While the eye-movement theory has proved to be inadequate, either as a necessary, sufficient or contributing condition, to explain all of the numerous cases to which it has been applied, it might appear that surely here, in the perception of movement from stationary stimuli, eye-movements must play a decisive rôle. It is not surprising, then, that the eye-movement theory should have been proposed for stroboscopic movement as early as 1886 by Fischer,[1] followed by Stern in 1894,[2] and lately by Higginson.[3] Higginson does not, however, offer it as a general condition underlying all seen movement, but merely as an assumption which most adequately explains a number of observations that he has made.

While the writers believe that a large number of facts in the literature (some of them reported by Higginson himself) effectually dispose of the eye-movement theory as an explanation of the phi-phenomenon, and that any theory must account for *all* the facts, not merely a few which seem to be explained by it, it seemed necessary to obtain direct experimental evidence of the part played by eye-movements during the perception of apparent

*Accepted for publication March 16, 1929.

[1]O. Fischer, Psychologische Analyse der stroboskopischen Erscheinungen, *Phil. Stud.*, 3, 1886, 128-156.

[2]L. W. Stern, Die Wahrnehmung von Bewegungen vermittelst des Auges, *Zsch. f. Psychol.*, 7, 1894, 321-386.

[3]G. D. Higginson, The visual apprehension of movement under successive retinal excitations, this Journal, 37, 1926, 63-115; The place of ocular movements in stroboscopic perceptions, *ibid.*, 408-413.

movement, before attempting a refutation of Higginson's position. On the other hand, in denying the validity of Higginson's explanation of the facts he believes the eye-movement theory satisfactorily explains, it does not necessarily devolve upon us to offer a substitute hypothesis that shall explain all the facts of seen movement even though the theory which is finally accepted must do this. In this paper we hope to show: (1) by photographing the movements of the eyes during the perception of the phi-phenomenon, that eye-movements do not play an essential rôle in the perception of movements—that, on the contrary, seen movement may be better when eye-movements are absent; (2) that the facts offered by Higginson in support of the eye-movement theory can be explained without recourse to a theory which leaves so many other facts unexplained and from which we cannot predict a single case; and (3) that a number of crucial phenomena reported by Higginson and others make the eye-movment hypothesis absolutely untenable as a general explanation of the facts.

Technique. The photographic technique, modified from that of Dodge, was similar to that used by Guilford in an earlier study.[4] A motion picture film was used in place of the panoramic film in the earlier study. It was moved by means of an electric motor at a rate of speed which could be controlled by means of a rheostat and reducing gears. The usual speed of the film was 8 mm. per sec. It was necessary to secure a simultaneous record of eye-movements and of the stimuli giving rise to apparent movement, upon the same film. In order to do this, a flashlight which threw a pin-point image upon the film beside the point-image from the eye was placed in circuit with the stimulus lights, going on and off with them. There are thus two records on the film as seen in Plate I: the eye record on the right, and the stimulus records on the left which appear as pairs of dots since they were flashed on momentarily with an interval between in order to produce the perception of movement.

The stimuli were controlled by means of a device which is new in studies of the phi-phenomenon. We have felt that the usual stroboscopic or tachistoscopic arrangements are unsatisfactory because the stimuli are not, though they are said to be, 'really' stationary. The usual arrangements possess an inner movement. In the common form of apparatus, a revolving disk with an open sector serves as a shutter for the light source. As a consequence the stimuli are not exposed as a whole all at once. The illumination begins at one edge, travels across to the opposite edge, and ends in the same way. Although this physical movement is not observable with the usual rapid speed of the shutter, it is a condition which may well be avoided. We are aware

[4] J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, this JOURNAL, 38, 1927, 557 f.

of the fact that the direction of apparent movement may be independent of the order of exposure of each stimulus and may, indeed, be directly opposed to it, yet we can speak properly of stationary stimuli only when they are exposed *in toto* at once and are cut off in the same way. Our technique provided such stationary stimuli and also made much easier the simultaneous recording of eye-movements and stimulus-objects upon the moving film.

The light sources for the stimulus-objects were two ordinary 2.5 v. flashlight bulbs. These were mounted in separate sections of an ordinary cigar box. The cover of this box was replaced by a black cardboard with two circular holes 1 cm. in diam. and 10 cm. apart. The holes were covered with one thickness of white bond paper and the lights were placed directly behind them.

Flashlight bulbs have an advantage over the usual 110 v. bulb because in the former there is little or no inertia. While we attempted to use neon bulbs whose physical inertia is almost nil, they were not adaptable to our needs, although further work in adapting these lights to the problem will no doubt bring success. We found the flashlight bulbs satisfactory for our purposes.

A rotary switch for controlling the stimulus-lights was made from an old phonograph. The phonograph motor has a fairly constant speed and hence makes an excellent timing device. It was this rotary switch which turned the currents for the stimulus-lights on and off while at the same time the signal light made a record of the appearance and disappearance of the stimulus-lights.

The phonograph disk-platform was stripped of its felt covering, exposing the metal base which was sandpapered until it was smooth, after which it was covered with a heavy paper in which open sectors had been cut. Contact brushes which rested upon the surface of the disk made electrical contact when they touched the open sectors as the disk rotated. The sectors were 1.2 cm. wide and extended arc-wise to the extent of 22.5° of the circular disk. The contact brushes were flexible strips of brass 0.8 by 7.0 cm., held at the upper end by a clamp upon a glass (insulator) rod which extended above one radius of the disk. There were two contact brushes for the two stimulus-lights. The sectors for these were so cut that the interval, in terms of degrees of arc, between the first and second contacts could be varied from 15° to 82.5°. The two extremes, 15° and 82.5°, were used almost exclusively, the former for stationary simultaneity, the latter for either full movement or succession. The time relations could also be varied by changing the speed of the phonograph motor. The speeds used varied between 50 and 90 revolutions per min., but speeds between 70 and 80 were most frequently used. The signal light which gave a record on the film of the stimulus-lights was governed by a contact brush used upon the open sectors responsible for the stimulus-lights.

The stimulus-lights were arranged for movement in the horizontal direction since the important component of the eye-movements which registers on the film is the horizontal one. The *Os* sat at a distance of 160 cm. from the stimulus-lights. A dim red fixation light, 0.2 cm. in diam., was set half-way between the stimulus-lights and 3 or 4 cm. above them. Two special arrangements were used in controlling the presentation of the stimulus-lights. By means of a throw switch in circuit with the phonograph disk, the circuits to the stimulus-lights could be made or broken whenever *E* desired. Thus the stimuli could

be given in succession just once, a mode of presentation we shall call single observing (S.O.) (*Einzelbeobachtung*), or they could be repeated, a pair on every rotation of the phonograph disk, a mode of presentation which we shall call multiple observing (M.O.) (*Dauerbeobachtung*). We shall use the abbreviations, S.O. and M.O. respectively, for these two types of presentation. The same throw switch opened or closed the circuit to the signal-light simultaneously with that for the stimulus-lights. The second special arrangement consisted of a commutator switch by which the order of stimuli could be reversed. We were thus able to determine the direction of movement, either from right to left (R–L), or from left to right (L–R). This change prevented the development of definite expectations on the part of the *O*s, thus preventing anticipatory movements as well as strong figural after-effects.

Observers. The *O*s were W. E. Walton (W), assistant in psychology; K. W. Braly (B), and Edwin Newman (N), graduate students in psychology, and the writers, (G) and (H). The work was performed at the University of Kansas during the spring and summer sessions of 1928.

Procedure. The procedure in the preliminary experiments was to adjust the speed of the phonograph disk until *O* reported optimal movement, and then to present series of stimuli while his eye-movements were being photographed. He was instructed to maintain his fixation on the red fixation-spot. The preliminary results were immediately convincing, for very rarely were there significant eye-movements simultaneous with the presentation of the stimuli. It was difficult, however, to obtain a time relationship for the stimuli which would give phenomenal movement 100% of the time. On the other hand, there were eye-movements along with some stimuli and not with others. We could conclude that eye-movements are not absolutely essential for the perception of the phi-phenomenon but we could not say that they had nothing to do with apparent movement. It was necessary, therefore, to distinguish between those stimuli which gave apparent movement and those which did not. To this end we gave *O* a telegraph-key in circuit with the signal light and asked him to report by pressing the key whenever he saw movement. With considerable practice the *O*s were able to do this with sufficient accuracy for our purposes. These reports may be seen in Plate I as heavier and longer dots and dashes following (below) each pair of stimuli in the S.O. series, and after every group of 5 pairs in the M.O. series. In the latter series such a report means that *O* saw movement in more than half of the group of 5. Those who have observed in this kind of experiment will know how difficult it is to decide in a moment's time whether or not movement occurred. The various stages, succession, part movement, full movement, dual part movement, and simultaneity merge so gradually into one another that it is hard, even with plenty of time, to name the class to which any single case belongs. From the protocols of the *O*s we are probably safe in assuming that a report of movement means a full movement, since in our preliminary experiments we determined the optimal conditions for full movement. Some of the reported movements may, to be sure, include some dual part movements, but since Higginson explains all these cases by his eye-movement theory, they should properly be included with the full movements.

A single series of observations consisted of forty stimulus-pairs presented in S.O. and then forty pairs presented in M.O. and arranged as follows so far as order of stimuli is concerned: 5 R–L, 10 L–R, 10 R–L, 10 L–R, and 5 R–L. This arrangement was not adhered to strictly, a fact which helped further to prevent anticipation on the part of the *Os*. Two, and sometimes 3, such series were presented in a single experimental hour with rests between them. In the later experiments the speed of the disk was adjusted so as to give full movement about half of the time, and something nearer succession the rest of the time. By doing this we had identical physical conditions, but sometimes the *Os* got phenomenal movement and sometimes not. If eye-movements are responsible for the apparent movement, or have anything to do with it, they should be present in great numbers in the former case as compared with the second case. We have counted the number of eye-movements in the case of 3 *Os* who served in the later experiments when phenomenal movement was reported and when it was not, as will be seen in Table I.

RESULTS

Table I shows the number of eye-movements which occur along with those stimuli which give phenomenal movement

TABLE I

PERCENT OF STIMULI GIVING MOVEMENT ACCOMPANIED BY EYE-MOVEMENTS

O		Movement Reported		No Movement Reported	
		Percent	p.e.	Percent	p.e.
G	S.O.	14.0	3.1	16.1	4.5
	M.O.	14.3	4.5	——	——
N	S.O.	7.3	3.3	12.8	2.6
	M.O.	15.7	3.4	11.1	3.5
B	S.O.	31.8	3.0	34.6	4.5
	M.O.	31.2	3.5	25.6	4.6

(first column) and those which do not (third column) during both the S.O. and M.O. presentations. The differences are very small, rarely, if ever, greater than the probable error of the difference, and they are negative as well as positive. There is absolutely no correlation between phenomenal movement and eye-movements. In fact, there seems to be a better chance for phenomenal movement, during the S.O. series, when the eyes do not move than when they do. This agrees with the observation of G that following the stimuli voluntarily with the eyes tends to destroy the phi-phenomenon and to give a rather 'unformed' configuration. But even in the S.O. series the negative differences are too small to be significant.

We next present the average duration of stimuli (e) and time interval between stimuli (p) which gave optimal movement for

the different *O*s, in Table II. While the interval p may seem long for optimal movement, in view of the value of 60σ given by Wertheimer as the interval for best movement for most of his *O*s, it must be remembered that p depends not only upon e but also upon the distance between the stimuli and the intensity of the

TABLE II

SHOWING IN σ, AVERAGE DURATION OF STIMULI (e) AND TIME INTERVAL BETWEEN STIMULI (p), GIVING OPTIMAL MOVEMENT FOR DIFFERENT *O*s

O	e	p
N	48	173
B	56	207
W	49	176
H	54	203
G	54	203

stimuli. According to Korte's laws,[5] there is an inverse relationship between the intensity of the stimuli, i, and the time interval, p, necessary for apparent movement. Since our stimulus lights, whose intensity was controlled by means of a rheostat, were adjusted at a rather low intensity, we should expect the interval p to be greater than it might well be under different conditions.

Protocols. We next present a few protocols from the *O*s. The impression of the *O* is generally that his eyes follow the moving light across the field. This is the conviction of the more unpractised *O*s especially, and is more true for the M.O. series than for the S.O. series. This is to be expected because in the M.O. series the stimuli come at regular intervals and may therefore be anticipated in spite of attempts to overcome this tendency on the part of the *O*s. Note the following protocols and then compare with the records and our explanation of them:

W "I was aware of an eye-movement. In spite of the fact that I tried to keep my eye upon the fixation-point, I had the impression that my eyes would jump to the first spot and move across to the second. I do not believe my eyes left the fixation-spot in spite of this feeling."

N "I thought that sometimes my eyes wandered at the first of the series of five exposures but I was sure they never did during the last one or two trials." (*E.* "What kind of wandering was it?") "I felt that when the light came on I looked at it and followed it across." "In one of the M.O. series I thought I felt eye-movements between stimuli. These were distinct from the kinaesthesis or strain which has always accompanied the apparent movement, that is, my eyes seemed to move in front of that strain as a background which I always feel."

B, "Even though I tried to fixate the red light I felt some eye-movement. I thought that my eyes moved almost with the light, but they couldn't move quite fast enough."

[5] A statement of these laws and their implications will be found in H. Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 509.

PLATE I
The records should be read from the top of the page down.
For explanation of the records see the text.

It will be seen that our *O*s report the kinaesthesis and strains which Higginson's *O*s reported. The only court of appeal, then, is the objective record of what the eyes were actually doing at the time the movement was seen, unless one resorts to the theory that the phi-phenomenon is due not to overt eye-movements but to strains and *incipient* movements. Apart from the fact that such a theory is open to many of the objections we shall urge against overt movements, it is a theory which hardly anyone would wish to defend, first, because it is not open to observation and experimental test, and secondly, because it suffers from vagueness and ambiguity in leaving the whole question in the dark as to the amount of strain or incipient movement necessary in the production of large, small, dual part and full movements. We therefore leave the question of unobservable movements and proceed to an examination of the records.

Plate I is to be read from the top down. Record A was taken without recording the occurrence of the stimuli. The periodic tremors in the line occur at the correct interval to be associated with the stimulus. The presentation was M.O. The tremors are undoubtedly *head-movements*, not eye-movements, for this record was made before a rigid biting board was provided. After securing the *O*s' heads no such records were obtained.

Record B shows just how far the eyes would have to move to satisfy Higginson's theory because here *O* attempted to follow the stimuli with voluntary eye-movements. *O* first fixated the red fixation-point, then he looked at stimulus A when it came on and finally at stimulus B when it appeared. If one compares this record with the remaining records where the *O* held his fixation as instructed, it will be noticed that while movement was seen, there is neither enough movement of the eyes nor movement in the proper directions to account for the seen movement.

Records C and D were made during S.O. series, the stimuli being so set as to give movement about half the time. Records E and F show what occurred during M.O. series.

Our conclusion is that in spite of reports of eye-movements, strain and kinaesthesis, the corresponding photographic records show no evidence whatever of eye-movements of such significance either in amount or direction to correlate with seen movement. The distinct kinaesthesis which one feels as the

two lights are flashed on in succession must be merely shifts in tonicity as the stimulus-field changes. Indeed, we have as much right to assert that the perception of movement is responsible for the felt kinaesthesis as to say that kinaesthesis or strain is responsible for apparent movement!

Higginson has given nine classes of facts which he believes are explained by the eye-movement hypothesis,[6] neglecting a number of facts not only in the literature from the work of others but also facts which he himself has reported which are fatal to this hypothesis. We shall first take up the facts he advances to support the eye-movement theory, after which we shall proceed to a discussion of the other facts which, according to the canons laid down by Higginson for any satisfactory theory, also have a claim upon our attention.

(1) *Os report ocular movement.* Introspections in which kinaesthesis from the eyes are reported support this statement. Yes, eye-movements may be reported, but are they present? Do they take place at the proper time and in the proper direction and are they large enough to explain the movement *actually seen*? Our photographic records show that they do not.

(2) *Presence of intermediate members.* The time interval between the exposure of the first stimulus and the second may be made not only too short for eye-movement from one stimulus to the other, but altogether too small for several successive fixations. Furthermore, the eye is blind during eye-movement and if the eye happened to be in motion before the appearance of the first stimulus, it would be impossible not only to see movement, but even more, the intermediate members described by Higginson.

(3) *Movement of negatively colored objects.* Higginson attributes this to change in fixation which give rise to negatively colored after-images. But Higginson does not state explicitly how he conceives eye-movements (change of fixation) can be responsible for this phenomenon. If the eye shifts its fixation from A (first stimulus) to B (second stimulus) while stimulus A is still acting upon the retina, this would make stimulation A cross over some retinal elements giving rise to seen movement, not in the direction of B, but away from it. If the eye does not shift in the direction of B until after stimulus A is cut off, then as the eye shifts to B, assuredly, the projected after-image of A would move across to B. But it is rare to get a negative after-image in the time given, and after such brief stimulation, the positive after-image is the one that should appear.[7]

[6] Higginson, *op. cit.*, 37, 1926, 408 ff.

[7] Fischer pointed out that the after-image cannot be the negative after-image because of the shortness of the time of exposure and the time interval between stimuli. In no case was he able to get a negative after-image in stroboscopic exposures. Higginson in the first article cited in footnote 3 (p. 65) wrongly interprets Fischer to mean that the negative after-image is necessary for the perception of movement in the stroboscope when he translates *Phasen* as after-image. The following paragraph from Fischer (*op. cit.*, 148) proves our point conclusively: "Es lag nun nahe, zu untersuchen, ob die Nachbilder, welche die stroboskopischen Erscheinungen vermitteln, unter Umständen complementär ausfallen können. So zahlreiche Versuche aber auch in dieser Hinsicht mit verschiedenfarbigen Punkten von mir angestellt worden sind, so habe ich doch in keinem einzigen Falle ein complementär gefärbtes Nachbild beobachten können. Der Lichteindruck ist bei allen diesen Erscheinungen so kurz, dass nur ein gleichfarbiges Nachbild entstehen kann."

Higginson does not state explicitly which of the above two interpretations is to be given to his theory. The first interpretation cannot, on purely physiological grounds, give rise to movement in the required direction, and is open to the very serious objection involving the time element as well as the resulting kind of after-image which should appear in the time available.

(4) *Movement beyond the second member.* Higginson states that the movement overshot the second stimulus and subsequently withdrew to it before disappearing, or the second stimulus was forced out of its position. In these cases the observer saw "a weak line where no line could possibly exist." This is no more mysterious than seeing apparent movement between discrete stimuli. We may answer this argument in Higginson's own words used in another connection, "one cannot argue directly from the physical order to the phenomenal." Yet Higginson demands eye-movements to save the constancy hypothesis! We too have heard *O*s exclaim at the great amount of 'overshooting' of the movement beyond the second stimulus object. This type of movement may be related either to *gamma* or *delta* movement. If *delta* movement, it can be explained on the basis of the conditions worked out for *delta* movement by Korte. Certainly at present it remains a most mysterious affair with no promise of being cleared, since, if eye-movements are not responsible, Higginson has no hypothesis whereby this fact of apparent movement can be linked with the large body of facts resting on fairly sound theoretical and experimental basis.

(5) *Dual movement.* Dual movement is cited by Higginson as further proof of his hypothesis. This type of movement is also explained by means of the bolstering hypothesis of after-images, a theory which was effectually disproved as early as 1912 by Wertheimer to whom we refer the reader.

(6) *Spatially dislocated lines.* Oblique lines may be seen as horizontal or *vice versa*. But how do eye-movements explain this finding? We are not told by Higginson. Moreover, the same phenomenon is found in apparent movement on the skin and hence we need an explanation that will carry over from one sense modality to another. We suggest that a spatially dislocated line may be due to the following factor: orientation in the dark (on the skin under other conditions) is normally very poor. It is affected (as *O*s have found in the Aubert phenomenon) by such factors as visual streaming phenomena, reported by one of the writers in his study of the autokinetic illusion, where it was found that eye-movements, on the contrary, favor good orientation or restore correct orientation which has been lost during steady fixation.[8] We should therefore regard spatial dislocation as a special case of the more general disturbances in spatial reference found in viewing a restricted field with no anchorage points or when only single or small parts of the field are illuminated, the rest being dark.

(7) *Momentary disappearance of the first member during movement of the second.* This may be a case of dual part movement where one stimulus moves, disappears, and the second takes up the movement. Or it may be due to

[8] J. P. Guilford, Autokinesis and the streaming phenomenon, this JOURNAL, 40, 1928, 401-417.

momentary inhibition of the first by the second which appears suddenly. In all of these cases Higginson has failed to state precisely what his conditions were and to control the mental set of his *Os*.

(8) *Movement of a single line.* This must be sought in something besides eye-movement because our records show the eye is still at the time of the appearance of the first stimulus. The direction of the movement will be determined by cortical set (used in a definite sense) as pointed out by Wertheimer[9] and DeSilva.[10]

(9) *Phenomenal differences in intensity, brightness and extent of stimulus objects.* When both stimuli were given simultaneously the *Os* reported that the stimuli were equal in brightness, extent and intensity, but when presented successively one member might be apprehended as more intense or brighter than the other. If the eyes move, all is made clear, we are told, but how it is made clear we are not told. Here again, we have an example of the constancy hypothesis in extreme form, for it is assumed that lines presented simultaneously and appearing equal in various respects should appear the same when presented successively. Factors other than changes in fixation or eye-movements may very well be responsible for this state of affairs, not the least of which may be mentioned is the fact that after the exposure and apprehension of one stimulus, both retinal and central processes undergo considerable modification which is bound to influence the appearance of the second stimulus, if it is seen.

Now there are further reasons why the eye-movements theory fails to meet the demands made upon it, both from the point of view of theory and of fact, of which Higginson was aware since he has stated what we expect of a good theory and has himself reported observations which are fatal to the eye-movement hypothesis. Nor was he unaware of the checks made by Wertheimer against eye-movements, which he reported in 1912[11] and which we reproduce here.

(*1*) The total time of the seen movement (from the exposition of the first stimulus to the disappearance of the second) was less than 100σ whereas 130σ are necessary for eye-movements.

(*2*) When the fixation point was held, the seen movement was just as good as when the eyes were allowed to move over the field during the exposures.

(*3*) Check against eye-movements was made by projecting after-images in which very slight movements could be detected—they remained stationary while apparent movement was good.

(*4*) The after-image of the first stimulus remained after the exposition of the second, holding its place, showing no movement had occurred.

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[9]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265. The six points made by Wertheimer against the eye-movement hypothesis which we reproduce below will be found on pp. 181-185 of the article cited here.

[10]H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, this JOURNAL, 37, 1926, 469.

[11]*Op. cit.*

(*5*) Movement in opposed directions simultaneously in the same field disposes also of incipient, unobservable eye-movements, as well as the hypothesis of movement of after-images.

(*6*) Real movement and the apparent movement seen in the moving picture theater cannot be satisfactorily explained by the eye-movement hypothesis.

In an article published a month earlier than the second one containing his eye-movement hypothesis,[12] Higginson had urged as an objection against the configurational theory of apparent movement that it did not explain the apprehension of movement in different directions seen simultaneously. Needless to point out this objection is even more fatal to the eye-movement hypothesis as advanced by Higginson. Moreover, in his first article on movement, Higginson described conditions under which it is possible to see apparent movement from stationary stimuli when the temporal interval between the two stimuli is zero. He tells us that with zero interval "for the first time we fail to find any report of 'no movement'. Full movement may occur under phenomenal simultaneity." In these cases Higginson would have to assume that even though his *Os* were instructed to maintain their fixation, every time the first stimulus was exposed the eyes were already in movement since no movement could be initiated in a zero interval. This assumption hardly seems plausible when we consider his statement that with zero interval *Os* perceived full movement *every time.*

We find, therefore, in studies which have appeared since the publication of Wertheimer's 1912 article, and in Higginson's own published reports, that there existed a number of observed facts and adequate checks sufficient to make the eye-movement hypothesis absolutely untenable. Higginson knows what the requirements of an acceptable theory are. He says,[13] "The final test of any scientific assumption is experimental verification. If the facts of *extended* observation (italics ours) fail at substantiation, a doctrine can have little worth." We may add that a theory which leaves the negative cases out of account is worthless. Why did Higginson fail to consider, in his presentation of his eye-movement hypothesis, facts which were in the literature before his time, facts which he himself believed to be crucial for any theory and which he urged against other theories?

In view of the large amount of work done on phi-phenomenon on the skin and in audition, the time has long since passed when a theory of apparent movement from stationary stimuli can lay claim to serious consideration which does not admit of application in more than one sense modality. It goes without saying that in the case of movement in 'bows' and arcs through the air above the skin and underneath the skin as reported originally by Benussi,[14] the assumption of something peripherally moving on

[12] G. D. Higginson, Apparent visual movement and the *Gestalt*, *J. Exper. Psychol.*, 9, 1926, pp. 228-252.

[13] *Ibid.*, p. 228.

[14] V. Benussi, Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psychol.*, 36, 1917, pp. 59-135.

the skin breaks down as badly as the peripheral explanation does in the case of visual movement. The finally acceptable theory of apparent movement will have to be, if not more complex, at least very different from any theory yet proposed, since some facts cannot be pictured according to known physical processes because nothing comparable to them appears to be known in electrical physics. Certainly our objective records which fail to reveal any correlation between eye-movements and seen movement, as well as the numerous other checks against eye-movements, make any such simple explanation as the eye-movement theory absolutely impossible as an acceptable explanation of the phi-phenomenon in its manifold variety.

Summary

In this article we have reported the results of some experiments in which eye-movements were photographed during the phi-phenomenon. No correlation whatever was found between eye-movements and the occurrence of apparent movement. If anything, eye-movements interfere with good phenomenal movement from two successive stationary stimuli.

Higginson's support of the eye-movement theory was critically examined and found to be without serious justification. Some of his own facts, as well as other facts reported since Wertheimer's study of 1912, cannot be explained by an eye-movement theory and they even tend to disprove any such theory for movement in general. Any general theory of apparent movement must take account of *all* the facts, not only of visual, but also of auditory and tactual movements from stationary stimuli.

SOME CONDITIONS OF THE PERCEPTION OF VISIBLE MOVEMENT

By Forrest L. Dimmick and Robert W. Sanders, Hobart College

During the past decade few single problems in psychology have received more attention than has the visual perception of movement.[1] It has formed the systematic rallying point of one school, certain aspects of it have been analytically described, new forms of it have been pointed out, and the influences of various factors have been studied. Numerous investigations have been concerned with the subject since the appearance of Wertheimer's observations in 1912.[2] Yet the fundamental questions remain unsettled. One must choose among several hypotheses or be content with various interesting details that have little systematic bearing on the main problem. On the other hand many of the articles are experimental and deal therefore with the facts of observation, and facts do not belong to any single system of psychology. Moreover, the stimulus conditions for all the experimental studies have been similar and the phenomenal criterion, the meaning of 'movement,' has been common to all of them. It should be possible, therefore, to bring all of the results into some sort of agreement, though of course, the systematic interpretations of them will remain diverse. In this experiment we set out to examine some of the factors concerning which there has been apparent factual disagreement, namely, the length of the temporal interval and the intensity of the stimulus.

Apparatus. Our experimental setting was essentially the same as that of the earlier investigations. The apparatus consisted of two projectors which were focused through an opening in one wall of a dark room upon a white screen 3 m. distant. The openings into the dark room were exactly fitted to the objectives of the projectors so that the latter were entirely outside and no light entered except that projected through the lens. Opaque slides, each having a narrow slit, one above and one below the center, were placed in the

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[1]Presented at the meeting of the American Psychological Association in New York, December 27-29, 1928.

[2]M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265.

slide carriers. The projected images of the slits were horizontal lines 1 cm. by 10 cm. and 10 cm. apart. The exposure times were controlled by a falling shutter placed at the nodal point of the optical system. The maximum error of this shutter was less than 5σ. The exposure time of each line was 30σ and the temporal intervals between the successive lines were respectively 0, 30, 60, and 90σ. The intensity of the stimulus was varied by interposing a ground glass at two different positions between the source of light and the slide. These gave stimulus intensities of (1) 'brilliant white,' (2) 'good white,' and (3) 'dull white.'

Observers. Our *Os* were three undergraduates majoring in psychology, J. C. Karl (K), A. Bacon (B), K. Cornwell (C), and the senior author (D). The first three *Os* had no knowledge of the problem, of the procedure, or of the apparatus of the experiment. While their general training in psychology included an understanding of the technical distinctions between 'process' and 'meaning,' the *Os* were not required to make this precise in their reports. They distinguished, however, almost invariably between what they 'actually saw' and what the experience 'seemed like,' 'reminded them of,' or 'meant.'

Instructions. The *Os* read through the following typewritten instructions at the beginning of every observation. "A stimulus will be shown you which will arouse a visual perception. Describe the perception as completely and as accurately as you can. Report no process of which you are not sure. The stimulus will be repeated to complete the description." The *Os* supplemented their verbal reports with sketches in which they indicated the spatial-qualitative pattern of the perception. Five series were presented with every combination of time interval and intensity. Prior to the regular series every *O* was given from 10 to 20 practice series until his reports became reasonably stable.

Results

Temporal interval. The reports of our *Os* fall under the principal rubrics worked out in the earlier investigation, viz., simultaneity, succession, and movement. The latter was either complete or partial, *i.e.* filled the whole field or only part of it.

Table I gives the percentage of reports by every *O* of the several perceptual experiences with every combination of time interval and intensity. Table II shows the frequency of the perceptions at the different times irrespective of the intensity of the stimulus. In the above tables the partial movement forms appear relatively infrequently and then mainly at the shorter intervals. On the other hand, complete or optimal movement is almost totally absent from the reports of the three principal *Os* when the temporal interval is 0σ. Those of D who worked with knowledge of the procedure and who therefore may have been predisposed toward the perception show 24% optimal movement reports in *one* set of observations. The great majority of reports at this

TABLE I

SHOWING THE PERCENTAGE OF THE VARIOUS PERCEPTIONS EXPERIENCED BY EVERY *O* WITH EVERY COMBINATION OF TIME INTERVAL AND INTENSITY

(Un = unimembral movement; Bi = bimembral movement; Op = optimal movement; Su = succession; Si = simultaneity)

Temporal interval		Observers B					C					K					D				
		Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
0	1	0	4	0	0	96	0	4	12	0	84	4	0	8	0	88	4	8	24	0	64
	2	0	8	0	0	92	8	8	12	0	72	0	0	4	12	84	0	0	0	0	100
	3	0	8	8	0	84	16	0	0	0	84	16	0	4	20	60	27	0	3	0	70
30	1	0	0	4	4	92	8	0	20	48	24	4	8	24	52	12	28	12	44	16	0
	2	0	20	12	20	48	4	0	56	40	0	0	0	48	32	20	20	0	60	20	0
	3	4	0	36	60	0	0	0	80	12	8	0	0	40	28	32	7	0	80	13	0
60	1	0	0	60	40	0	0	4	52	44	0	0	0	80	20	0	12	8	64	16	0
	2	4	0	52	44	0	4	0	76	20	0	2	0	56	32	0	0	20	60	20	0
	3	0	0	48	52	0	0	0	100	0	0	0	0	52	48	0	0	0	100	0	0
90	1	0	0	32	68	0	0	0	76	24	0	0	0	84	16	0	20	12	20	48	0
	2	0	0	40	60	0	0	0	36	56	8	4	0	92	4	0	0	20	40	40	0
	3	0	0	56	44	0	0	0	24	64	12	0	0	72	28	0	20	20	0	60	0

TABLE II

SHOWING THE EFFECT OF TEMPORAL INTERVAL UPON THE REPORTS

(For an explanation of the abbreviations see Table I)

Temporal interval	Observers B					C					K					D				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
0	0	7	3	0	90	8	4	8	0	80	7	0	5	11	77	9	4	20	0	67
30	1	7	17	28	47	4	0	52	33	11	1	3	38	37	21	20	7	58	15	0
60	1	0	54	45	0	1	1	77	21	0	4	0	63	33	0	8	8	69	15	0
90	0	0	43	57	0	0	0	45	48	7	1	0	83	16	0	17	15	17	51	0

time interval are of simultaneity. When the temporal interval is increased to 30σ, optimal movement appears relatively frequently, simultaneity decreases, and succession appears. With further increase of the interval simultaneity disappears and optimal movement increases. At the 90σ interval, succession increases at the expense of optimal movement.

These results do not of themselves settle the question of the dependence of optimal movement on the length of the temporal interval, but they do add evidence to one side. Wertheimer reports that the shorter intervals give simultaneity,[3] the longer ones, succession, and the intermediate ones movement. Our earlier studies agree with these findings and give a quantitative confirmation of this relation between the three perceptions and the temporal interval. Higginson,[4] on the other hand, reports the "apprehension of movement" of the complete type in some 85% of his presentations, irrespective of the temporal interval. An interval of oσ is as efficacious as any other. It should be possible, now, to duplicate this later set of results with our *O*s, but first we must clear up certain differences of interpretation. So far as we can judge, the phenomenal aspect of the perception with which we are dealing is the same as Wertheimer's. That is, the movement is visible, or seen, even though his *O*s (or ours) report no visible object which moves. Our *O*s sometimes make the explicit statement that movement is "meant" when it is not "seen," but such cases are ordinarily excluded from their reports by the instruction: "Report no process of which you are not sure." As an example:

B, [90σ, intensity 1]: "A line appeared near the center of fixation then it moved down—No! it didn't move!—I couldn't *see* the movement but another line appeared below and the first disappeared."

As over against this, Higginson embraces under his heading, "full movement," every apprehension of movement that is referred to the whole field whether or not it is seen in that area. This difference of interpretation undoubtedly accounts for some degree of the preponderance of "full movement" in his results.

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[3] *Op. cit.*, 178.

[4] G. D. Higginson, The visual apprehension of movement under successive retinal excitations, this JOURNAL, 37, 1926, 63-115.

There remain, however, the large number of simultaneities and successions in Wertheimer's results and in ours that are absent from Higginson's. These observations are prior to the interpretations and demand further experimental examination. With this in mind, we altered our procedure. The crucial point, it seemed to us, might lie in the *Os*' attitude. Consequently, we altered our instructions to read, "You will be shown a stimulus which may or may not arouse a perception of movement. State whether you perceive movement. If you do, characterize the movement in every way that you can." No change whatever was made in the stimulus conditions. Table III shows the effect of the predisposition to perceive movement upon the *Os*' reports. The percentage of perceptions of movement for all *Os* is more than doubled. We are justified in concluding, therefore, that some factor operated in the observations of Higginson's *Os* to precisely the same end as the attitude set up by our second instructions. If, then, we are concerned with the phenomenon of *visible* movement, which is conditioned primarily by a set of stimulus conditions and not by an attitude of the *Os*, the temporal interval is an essential factor for the determination of the perception. On the other hand a predisposition to perceive movement will nullify the effect of any single stimulus factor or group of factors, and the meaning, "movement of a visual object," or just "visual movement," may be carried by a context which may include any sort whatever of sensory datum or it may be implicit in the situation and taken for granted. The last two forms of the perception do not come within our understanding of the term 'visible movement.' For the sake of labeling them we may call them "apprehension of movement from visual stimuli" and "inference of movement."

TABLE III

SHOWING THE EFFECT OF PREDISPOSITION UPON THE REPORTS
(For an explanation of the abbreviations see Table I)

Time interval	Observers B					C					K				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
0	0	0	100	0	0	0	0	87	0	13	0	0	47	0	53
30	0	0	100	0	0	13	0	87	0	0	0	0	73	0	27
60	7	0	93	0	0	0	0	100	0	0	0	0	93	7	0
90	0	0	100	0	0	0	0	87	13	0	0	0	67	33	0

Intensity. The second factor upon whose influence we hoped to throw light is intensity. We took Korte's "intensity" to mean intensity of light, and we tried to approximate his conditions in our projection apparatus by having three intensities of the projecting light.[5]

TABLE IV

SHOWING THE EFFECT OF INTENSITY OF STIMULUS UPON THE REPORTS
(For an explanation of the abbreviations see Table I)

Intensity	Observers B					C				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
1	0	1	24	28	47	2	2	40	29	27
2	1	7	26	31	35	4	2	45	29	20
3	0	3	37	39	21	4	0	51	19	26

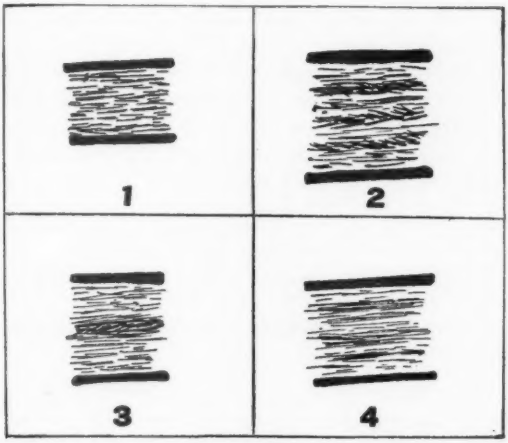
Intensity	Observers K					D				
	Un	Bi	Op	Su	Si	Un	Bi	Op	Su	Si
1	2	2	49	22	25	16	10	38	20	16
2	4	0	50	20	26	5	10	40	20	25
3	4	0	42	31	23	12	5	50	18	15

Tables I and IV give indication of some influence of the intensity of the stimulus on the perceptual experience. Table IV shows the percentage of the various perceptions as they depend upon intensity irrespective of the temporal interval. From this table we should be forced to conclude that intensity is not a factor in the perception. If we turn back to Table I, however, we find that at a given temporal interval the frequency of optimal movement varies with the intensity of the stimulus, though not unequivocally. In the main, with temporal intervals of 30σ and 60σ optimal movement increases with increase of intensity. At 0σ and 90σ intervals changes of intensity produce little or no change in the perception. Thus we cannot confirm Korte's work in detail, but neither do we dispute it. The condition, we believe, is not as simple as he makes it.

The existential correlate of visible movement. So far we have been dealing with the phenomenal aspects of movement, and our results are, in the main, in accord with the results of the Gestalt school. We were concerned, further, with a more adequate description of the sensory carrier of 'visible movement,' which we

[5] A. Korte, Kinematoskopische Untersuchungen, *Zsch. f. Psychol.*, 72, 1915, 193-296.

have reported previously as a "flash of gray." The process-meaning relation of this 'gray flash' to visible movement, is as definite for the present set of *O*s as for Dimmick's *O*s at Cornell,[6] and Dimmick and Scahill's *O*s at Michigan.[7] The observational task is far from easy, so that a complete description must be built up from successive presentations. The following examples chosen from the reports of the several *O*s give characteristic observations of the qualitative aspects of the perception.



FIGS. 1–4. DIAGRAMS ILLUSTRATING THE DIFFERENT KINDS OF OPTIMAL MOVEMENT

SAMPLES OF REPORTS

Optimal movement. B, [60σ, int. 1, Fig. 1]: "A line appeared near the center of fixation. It changed position so quickly that it gave the effect of a series of parallel horizontal lines which, however, were indistinct and blurred together, and much grayer than the line itself. I could see the movement down to the lower position where the line appeared distinctly with a patch of gray above it."

C, [60σ, int. 2, Fig. 2]: "I saw a white line in two positions. It started at the top and moved to the bottom very rapidly giving the effect of hazy gray lines in between."

[6]F. L. Dimmick, An experimental study of visual movement and the phi phenomenon, this JOURNAL, 31, 1921, 317-332.

[7]F. L. Dimmick and H. G. Scahill, Visual perception of movement, *ibid.*, 36, 1925, 412-417.

K, [90σ, int. 2, Fig 3]: "I saw three lines. The middle one was less distinct than the first and last. Between were blotches of light gray. The impression I got was that a line moved from top to bottom."

D, [60σ, int. 1, Fig. 4]: "A line appeared in the first position with the field below it gray and lighter near the line. Then the second position came with the gray patch still above it. The perception was that of a line moving rapidly from one place to another. There were streaks in the gray which looked like the line in movement."

Simultancity. B, [0σ, int. 1]: "There were two parallel white lines near the center of the fixation field which came and went together."

C, [0σ, int. 1]: "Two parallel lines appeared rapidly and disappeared. They were simultaneous."

K, [0σ, int. 1]: "I saw two parallel light lines for a very short time."

D, [0σ, int. 1]: "Two horizontal lines came simultaneously. They flashed on and off and were stationary."

Succession. B, [90σ, int. 1]: "A line appeared in the middle of the fixation field. It disappeared and another appeared below its position and parallel to it. They were not together but in discrete succession."

C, [90σ, int. 3]: "That time there were two distinct lines; first one then the other in succession. There was nothing in the background."

K, [90σ, int. 3]: "I saw two quick flashes of light, one beneath the other and following it."

D, [90σ, int. 1]: "There were two successive lines. The first came and went and then the second did likewise."

There seems to be no doubt that the movement which these *O*s saw is qualitatively gray, tinged perhaps with the color of the stimulus lines. When the color of the stimuli was either red or green, the carrier of 'movement' remained predominantly gray. Sometimes it was slightly reddish or greenish but as frequently just gray. When one stimulus was red and the other green, gray predominated though it was sometimes tinged with one of the colors.

This 'gray' which is the fundamental existential correlate of visible movement has the following characteristics. (1) It is never a surface but varies from a true film toward a bulky color. The *O*s describe it sometimes as a "curtain" of velvety gray "without a definite surface." At other times it lies in three dimensions though the exact distances can not be stated. (2) Its localization is indeterminate. It lies somewhere near the plane of the stimulus line but it is never confined to it. The *O*s are never sure whether it lies before or behind that plane. Sometimes, as we noted above, it extends into space behind. (3) It is "un-

steady," "flickering,"[8] "light and dark in various parts but not in a fixed pattern." This instability shows no direction of change. (4) The area is a constant factor in any position, *i.e.* the size of the gray 'patch' does not change. (5) The temporal aspect is always noted as very brief.

As we have previously shown, the 'visible movement' set up by two discrete, stationary stimuli is perceptually indistinguishable from that resulting from a moving stimulus,[9] and their descriptive aspects are identical. In the case of the latter, *i.e.* the moving stimulus, the qualitative filling of the movement field may be ascribed to a positive after-image of the stimulus. This would be gray when the stimulus and background are black and white, and a low chroma of the stimulus color when the latter is colored. The source of the gray with the stationary stimulus is not so obvious. We may ascribe it to the ordinary tied image. Such, indeed, seems to be the explanation for the coloring which sometimes appears. We suggest, however, that the gray which forms the qualitative matrix of 'visible movement' is another instance in which 'central gray' is projected and becomes visible in the visual field. Its appearance and the conditions of its occurrence are comparable with those of Jaensch's "*Zwischenmedium*,"[10] and Möller's "glassy" perception.[11]

Our conclusions are in essential accord with the findings of another investigator who has attacked the problem from a very different angle. DeSilva examined phenomenologically the effect of a number of factors on the perception.[12] His conclusion that "the perception of movement is a complex, variable experience" gives substantiation to our tentative classification into 'visible movement,' 'apprehended movement,' and 'inferred movement.' The common term 'movement' does not guarantee that the experiential factors or the phenomena are the same. Higginson's,

[8]It is significant to note here that, whereas we describe movement as "flickering grey," the author of a recent phenomenological study of flicker concludes that flicker is essentially movement. Cf. Kurt Haack, *Experimental-deskriptive Psychologie der Bewegungen, Konfigurationen und Farben unter Verwendung des Flimmerphänomens*, 1927, 25 ff.

[9]F. L. Dimmick and H. G. Scahill, *op. cit.*, this JOURNAL, 36, 1925, 412-417.

[10]E. Jaensch, Ueber die Wahrnehmung des Raumes, *Zsch. f. Psychol.*, Erg. Bd. 6, 1911, 244-307.

[11]E. F. Möller, The glassy sensation, this JOURNAL, 36, 1925, 249-285.

[12]See footnote, p. 616.

DeSilva's, and our own results indicate that they are not. DeSilva points out in several places that an 'object consciousness' or 'meaning attitude' enhances the perception, that is, insures the presence and definiteness of the meaning 'movement.' Conversely, an analytic attitude or repetition of the stimulus dissipates the 'movement.' In addition the effects of background, fixation, and circumscription upon the *visibility* of the movement which DeSilva determined are what one would expect if, as we suggested, "central gray" is the basis of the 'gray flash' which carries the meaning 'visible movement.'

Summary

(1) From our results and their relation to those of other investigators in the field we contend that the conceptual term 'visual movement' is not adequately definitive either of a single mental phenomenon or of a single perceptual pattern. We suggest tentatively, the rubrics 'visible movement,' 'apprehended movement,' and 'inferred movement.'

(2) The basic sensory correlate of 'visible movement,' which may be identical with the phi-phenomenon, is an unsteady or flickering bulky-film of gray which in the case of the stroboscopic stimulus, consists primarily of projected "central gray" modified in a particular case by the qualities of the stimuli and of the background and by perceptual images of the order of 'memory color' or 'tied images.'

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[12]H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, this Journal, 37, 1926, 469-501; Kinematographic movement of parallel lines, *J. Gen. Psychol.*, 1, 1928, 550-577. See especially page 554, footnote 6, where DeSilva calls attention to the fact that in our last study of visual movement we discontinued the use of separate 'meaning' instructions. This methological change does not imply a confusion of the two types of report on the part of the *Os* after they had acquired adequate practice in observation. This practice was attained by sheer repetition of the stimulus. Reports at the beginning were confused and wholly inadequate on the descriptive side (see DeSilva). The *Os* were trying, as *Os* always do, to evade the descriptive task by finding out what the experimenter "wanted them to say." Failing in this, sooner or later all of them described the experience itself. They always made interpretative statements, however, about the descriptions they had given. This obviated the necessity of our taking a duplicate set of reports of meaning. Our first group of trained *Os*, on the other hand, did not give interpretations under the instruction to 'describe.' The similarity of the descriptive reports under such great differences of psychological sophistication argue against any artificiality of the existential data. In addition, DeSilva's *Os* are in essential agreement with our own.

AN EXPERIMENT ON AFFECTIVE CONTRAST

By Albert J. Harris, Harvard University

The law of affective contrast is, to the best of my knowledge, supported by the results of but one experiment, that published by Bacon, Rood, and Washburn in 1914.[1]

In this experiment a large number of *O*s made absolute judgments on the pleasantness or unpleasantness of colored papers, on a scale from 1 to 7. For some of the *O*s, a set of 18 colors was presented, first with each preceded by one of six previously chosen pleasant colors, then with each preceded by one of six unpleasant colors. For others the unpleasant colors were presented first and the pleasant colors afterwards. For *O*s working without knowledge, "every observer, who had the colors presented first after unpleasant and later after pleasant colors, had more affective values lowered than raised by the process; twenty-three out of twenty-five to whom the colors were shown first after pleasant and later after unpleasant colors had more affective judgments raised than lowered on the second presentation."[2]

The authors think that these results certainly demonstrate the influence of contrast, and formulate the law of contrast as follows: "The pleasure of an agreeable experience is heightened if it is preceded by a disagreeable experience, and an impression in itself unpleasant may be felt as pleasant if a more unpleasant state has been its antecedent. In like manner unpleasantness may be heightened or even created through contrast with a preceding agreeable affective state."[3] These results conform equally well, however, to a law recently proposed by Dr. J. G. Beebe-Center and termed the law of affective equilibrium. The law states that "the affective value of the experiential correlate of a stimulus

*Accepted for publication May 18, 1929.

[1] M. M. Bacon, E. A. Rood, and M. F. Washburn, A study of affective contrast, this Journal, 25, 1914, 290-293.

[2] *Op. cit.*, 292.

[3] *Op. cit.*, 290 f. The statement follows that these are laws "which everyday experience has established;" however, no other definition of affective contrast is given in the paper.

varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group."[4]

The question now arises as to which of these two laws is the more adequate to describe the phenomena usually referred to as affective contrast. The essential difference between the two laws is that the former makes the change in the affective value of a stimulus a function of the conscious event immediately preceding the application of the stimulus, whereas the latter makes it a function of all the conscious events preceding the application which constitute with it a unitary group. In order to determine which of these laws is the more adequate, it would seem to be sufficient to find out whether, in an experiment involving a contrast effect, the different judgments upon a single stimulus vary in a definite way with the affective value of the immediately preceding experience. The experiment described below is designed to answer this question.

Technique

The stimuli used were 90 Milton-Bradley colored papers, mounted on four-inch squares of white pasteboard. The *O*s were Messrs. K. W. Oberlin, W. D. Turner, and H. L. Kim, and Miss D. Selling, all research students in the Harvard labortory. They will be designated hereafter in this paper by the first two letters of their surnames.

The 90 cards were spread out on a table, and each *O* was asked to select the 10 most pleasant and 10 most unpleasant colors. This gave a P (pleasant) determining series and an U (unpleasant) determining series for each *O*. From the 41 colors that were left untouched by all four *O*s a representative series of 20 was selected by *E*. It was called the test series, and was the same for all *O*s.

A haphazard order of presentation was arranged, with determining and test colors alternated, so that every determining stimulus was followed once by each of the test stimuli. This order was kept throughout the experiment. The colors were presented serially against a background of medium gray. The exposure was 4 sec., and the interval between colors was 5 sec. The colors were approximately level with the eyes of the *O*, and about 4 ft. from him.

The instructions were as follows: "Colors will be presented to you one at a time. You are to make a report of their pleasantness or unpleasantness, judging them as +3 (very pleasant), +2 (pleasant), +1 (slightly pleasant), 0 (indifferent), −1 (slightly unpleasant), −2 (unpleasant), or −3 (very unpleasant). Judge each color independently, and avoid all memory or association effects."

The experiment proper consisted of two parts. For *Ob* and *Tu*, Part I consisted of judgments on the intermingled P series and test series, and Part

[4] J. G. Beebe-Center, The law of affective equilibrium, *ibid.*, 41, 1929, 64.

II of judgments on the intermingled U series and test series. For *Se* and *Ki* the U determining series was given in Part I and the P series in Part II. Each part (consisting of 400 judgments) was completed in three experimental sessions, one week apart. The first and third sessions in each part consisted of 120 judgments, and the second of 160 judgments. A rest of 5 min. was given after every 40 judgments. There was an interval of one week between the choosing of the determining series and the first trial of Part I, and an interval of three weeks between the last trial of Part I and the first trial of Part II.

Possible Sources of Error

One possible source of error of considerable magnitude is the attitude of *O* Bacon, Rood, and Washburn reported that *O*s with knowledge of the purpose of the experiment gave contradictory results. *E* tried to keep the *O*s ignorant of the purpose of the experiment, and of the nature of their results. Introspective reports obtained at the conclusion of the experiment showed that he was successful with all four *O*s.

Another possible source of error is the influence of memory of the preceding judgments. The *O*s were instructed to try to avoid this effect. In their introspective reports they agree that memory of a previous judgment on a color occurred in relatively few cases. When a memory did occur, *O* was able most of the time to make his judgment independently of the memory. It seems to the writer that the influence of memory on the results may therefore be regarded as negligible.

A third—and most serious—possible source of error is the arithmetic treatment of the judgments in the calculation of the results. To the writer this method seems the best available. Bacon, Rood, and Washburn found which way the majority of the shifts (obtained by presentation after pleasant and unpleasant stimuli) occurred in most of their *O*s. Their procedure is open to two objections. In the first place, the determining stimuli are not constant. The results of the present experiment show, on the one hand, that the pleasant determiners are not equally pleasant, nor are the unpleasant determiners all equally unpleasant; and, on the other hand, that a determiner usually called pleasant (or unpleasant) may occasionally be judged to be the opposite. A valid method of treatment must take this variability into account. In the second place, there is a question which concerns the size of the shifts involved. It seems to the writer that a shift from 'unpleasant' to 'very pleasant' must be treated as greater than a shift from, say, 'indifferent' to 'slightly pleasant.' The only way to take account of such differences is to use an arithmetic scale. Its use does not mean that the arithmetic values are taken to mean equidistant, constant points on a fixed scale. They are, in such a case as this, conventions for the convenient expression of similarities and differences that cannot otherwise be stated and compared except in a very awkward and roundabout way. The writer further believes that the use of these conventions brings out general tendencies in the results more clearly and accurately than any other method.

Results

(*1*) *Individual contrast.* The results from each *O* were treated separately. From the judgments made on the determiners the average affective value of each determiner was found, and the determiners were arranged in rank order of pleasantness. Every test stimulus was presented once after each of the determiners, so the average affective value of the test stimuli following a determining stimulus could be made the basis for comparing the effect of various determiners on the affective value of the test stimuli. The results for each *O* in Part I are shown in Fig. 1. If contrast is an individual effect, in the sense of depending solely on the immediately preceding experience, the average value of the test colors following the least pleasant determiners, those on the left, should be higher than the average value following the most pleasant determiners, those on the right. The results show no such tendency. The figures of the corresponding results in Part II are shown in Table I. It was felt to be unnecessary to give both figure and table for each part.

TABLE I

Showing the Average Value of the Test Colors Following the Different Determiners

The determining colors are arranged in rank order of pleasantness, with the least pleasant at the left. Braces indicate that two or more determiners had the same affective value.

O	Determiners									
	1	2	3	4	5	6	7	8	9	10
Tu	.45	.0	.85	.15	.80	1.10	1.10	−.45	.55	.60
Ob	−.25	−.05	.15	.15	.45	−.05	.25	−.35	.20	.50
Ki	.75	.25	.65	.65	.85	.75	.50	.80	.40	1.10
Se	.25	.10	.25	.0	.40	.20	.35	.0	−.05	−.15

Not too much reliance should be placed on these calculations, however, for they are based on the *average* value of the determiners. If the value of each determiner were constant, this method would be valid, but, since variability always occurs when colors are used as affective stimuli, the use of the average value of the determiners is unsafe. To avoid this difficulty another method of calculation was devised.

A table was constructed in which the value assigned at each presentation of a test color was shown under the value given to

the determiner which immediately preceded it. Two such tables were made for each *O*, one for the part with the U determining series, and one for the part with the P determining series. Table II shows a representative portion of one of these tables. The ideal procedure would be, of course, to compare the average value of the test colors following -3 determiners with those following $+3$ determiners. However, there were few $+3$ judgments given to P determiners, and few -3 judgments to U determiners. The

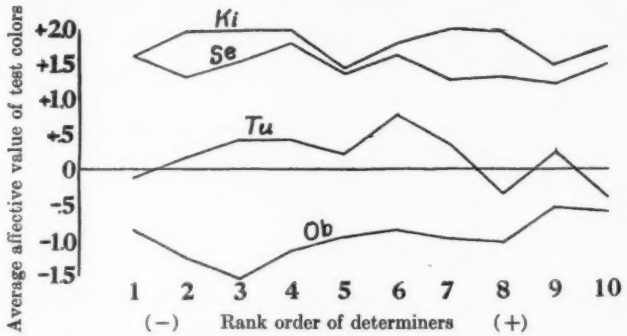

FIG. 1. AVERAGE VALUE OF THE TEST COLORS FOLLOWING THE DIFFERENT DETERMINERS

If there were individual contrast, the lines would tend downward from left to right.

average of a column with few figures, such as the -2, $+2$, or $+3$ columns in Table II, has little value, so it was necessary to select columns for comparison. In each of the eight tables the two

TABLE II

PART OF ONE OF THE TABLES SHOWING THE VALUE GIVEN TO EACH TEST COLOR UNDER THE VALUE GIVEN TO THE IMMEDIATELY PRECEDING DETERMINER AT EACH PRESENTATION

	OBSERVER—*Se*					DETERMINING SERIES—U	
Test	Determiner values						
colors	−3	−2	−1	0	+1	+2	+3
1	+1, +3		+3, +3, +3	+3, +3	+3, +3, +3		
2	0, +1		−1, 0, +2, 0	−2, +1	+1, +1		
3	−2		0, +1, +1, +1	+2, +1	+1, +1, −1		
4	+2	+2, +2	+2, +3	+2, +3	+2	+3	+1
5	+2, +1		+1, +1	+1, +1	+2, +2, +2	0	
etc.							

columns were selected which were farthest apart, and which at the same time included values for nearly all of the test colors. Thus in Table II the -3 and $+1$ columns were selected. Eighteen of the 20 test colors were represented in both of these columns. The other two were disregarded, and the average value of the 18 test colors in these two columns was taken; in this case, it was $+1.7$ in the $+1$ column, and $+1.5$ in the -3 column. The averages of the selected columns from each of the eight tables are shown in Table III. If there is an individual contrast effect, the right-hand figure of each pair of averages in this table should be lower than the left-hand figure.

TABLE III

SHOWING THE AVERAGE VALUES OF TEST COLORS FOLLOWING P AND U DETERMINERS, IN BOTH PARTS FOR ALL *O*s
(See text)

O	Determining series	Determiner values							Contrast
		−3	−2	−1	0	+1	+2	+3	
Tu	U		+.55				.28		yes
Ob	U		−.07			.13			no
Ki	U			.20				−.04	yes
Se	U	1.26				1.43			no
Tu	P		−.05					−.25	yes
Ob	P			−1.35				−.50	no
Ki	P					.79		1.05	no
Se	P					.20		−.04	yes

It is apparent from the table that, with a given *O* and a given set of determiners, the value of the test stimuli does not vary in any definite relation to the value of the preceding determiner. In fact, one *O* shows a slight tendency toward a contrast effect with both P and U determiners, one *O* shows the reverse in both cases, and the other two *O*s show contrast once and the opposite of contrast once.

TABLE IV

SHOWING THE DIFFERENCE IN THE AVERAGE VALUE OF THE TEST COLORS WHEN THE DETERMINING COLORS ARE CHANGED

O	Test average after P determining series	Test average after U determining series
Tu	0.15	0.50
Ob	−0.97	0.02
Ki	0.69	1.73
Se	0.01	1.42

This method of calculation fully confirms the results of the first method. In both cases the results show small deviations from a neutral state, and the distribution of these deviations is of the type to be expected of pure chance variations. It seems safe, therefore, to conclude that there is, in this experiment, no evidence of an individual contrast effect, *i.e.* of one depending solely on the immediately preceding experience.

(*2*) *Mass contrast.* An inspection of Table IV shows that, for three *O*s, the average value of the test stimuli is very markedly

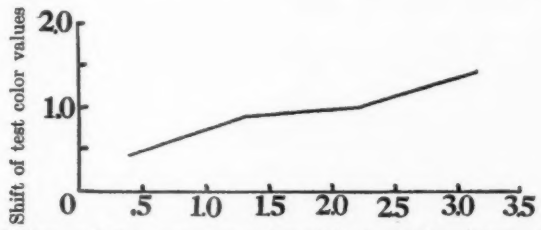

FIG. 2. SHOWING HOW THE AMOUNT OF SHIFT IN THE AFFECTIVE VALUES OF THE TEST COLORS VARIES IN RELATION TO THE DIFFERENCE BETWEEN THE TWO DETERMINING SERIES

higher with U determiners than with P determiners. In the case of the fourth *O*, (*Tu*) the same tendency is present, but it is not as marked as in the other three cases. The difference between the two determining series, as measured by the difference between the average affective values of the two series, was not, however, the same for the different *O*s. It seems natural that a large difference between the two determining series should bring about a greater shift in the test values than a small difference. An inspection of Fig. 2 shows that this is just what occurred. For each *O* the amount of shift in the test values is plotted vertically, and the difference between the two determining series horizontally. Not only is the shift in the test series larger the greater the difference between the determining series, but the four points yield a practically straight line. Approximately, the amount of shift in the test values varies proportionally with the difference between the determining series.

Conclusions

We have seen that there is no demonstrable individual affective contrast effect. On the other hand, the affective value of a series of stimuli is changed when another group of stimuli is interpolated. The value of the test series is higher when an unpleasant series of stimuli is interpolated than it is when a pleasant series is interpolated, and the amount of shift is proportional to the difference between the determining series. This might be called a "mass contrast" effect. It is adequately described by Beebe-Center's law of affective equilibrium: "The affective value of the experiential correlate of a stimulus varies conversely with the sum of the affective values of those experiences preceding this correlate which constitute with it a unitary temporal group."

The question as to just what stimuli can constitute a unitary group remains as yet unsolved. It may be that an equilibrium effect is possible with stimuli of different modalities. The writer hopes that experiments which he is now making will shed some light on this problem.

VARIATIONS TOWARD PURPLE IN THE VISUAL AFTER-IMAGE

By Theodore Karwoski, Harvard University

The correct appreciation of the phenomena of visual after-images has suffered from over-simplification. In general, psychologists believe in the existence of a negative after-image of exactly complementary hue, and in the existence of a positive after-image; and they know something of the standard conditions under which each of these occurrences can be induced. The belief in these simple, clearly defined phenomena has come about because the well-known theories of color have tended to fix norms and to provide no explanation for phenomenal deviations from them. The elementary text-books have also, undoubtedly for pedagogical reasons, ignored deviations from these two standard types of after-images, and then the belief, once established, has reënforced itself by tending to limit research to those conditions under which the accepted results are usually found. Nevertheless, the experts have always known that the phenomena of after-images are neither as fixed as the theories require nor as definite as the text-books declare. Helmholtz[1] recognized the existence of variations. Parsons[2] gives warning of the existence of variations. So do Troland[3] and McDougall.[4] The phenomena need to be studied with the more careful control of conditions that spectral light yields and under extremes of intensity as well as in the middle ranges.

The present study is designed to accomplish a survey of the phenomena of after-image at a variety of intensities of the stimulus and of the field of projection. It is confined to the use of monochromatic light because impure lights introduce unpredictable variations and lessen the precision of the results.

*Accepted for publication April 30, 1929.

[1]H. von Helmholtz, *Physiological Optics*, Amer. ed., 1924, II, 261.

[2]J. H. Parsons, *Introduction to the Study of Colour Vision*, 1915, 102.

[3]L. T. Troland, Preliminary note: The influence of changes of illumination upon after-images, this Journal, 28, 1917, 497.

[4]W. McDougall, Some new observations in support of Thomas Young's theory of light and color vision (III), *Mind*, N. S. 10, 1901, 17–18.

Apparatus and Procedure

The apparatus consisted of two units—a spectral light system and a white light mechanism. The light from both units entered the eye after traversing the same pathway. Along this course a diaphragm, shaped like a half-moon, eliminated part of the beams, so that the light was seen as a semi-circular image. This diaphragm was at a distance such that the image fell within the foveal area. The eye was equipped with an artificial pupil, 2 mm. square, which adequately controlled any changes that might have been caused by the pupillary action.

Fig. 1 illustrates the optical system employed. In it S is the spectral light source; W is the white light source.[5] The light beams from W are reflected from the magnesium coated disk, M, along the same course taken by the rays

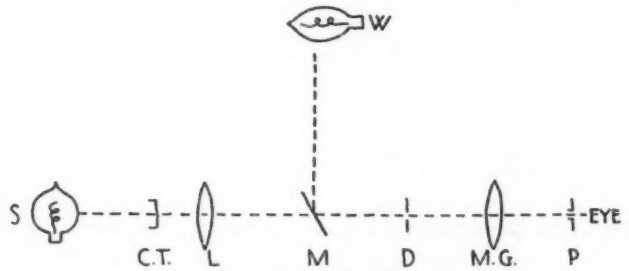

Fig. I. After-Image Apparatus

from S to the eye. C.T. is the tube of a spectrometer with its slit from which the divergent rays come. These rays are collected by the lens, L, and are changed into parallel beams. M.G. is a magnifying lens; P is an artificial pupil through which observations are made.

The experimental procedure was as follows. The magnesium disk could be moved out of the path of the beam from S by pulling a string which revolved the disk, M, into a position such that it was no longer in the path of the rays from S . After the eye had been fatigued with a spectral color, the disk was suddenly and noiselessly brought into the path of S, thus cutting off its radiations. The after-image immediately followed, projected on the magnesium disk as a field.

The apparatus contained supplementary features by which it was possible to measure photometrically the stimuli studied and also the lights used as reacting fields. Since the white light would vary in color with the incandescence of the lamp, Wratten Photometric Filters were used in such combinations so that all the white reacting fields matched Cambridge noon daylight.

[5] The light sources employed were as follows: S is Mazda (G.E.) specially gas filled lamp, 6 volts, 4 amperes; W is a Street Series lamp, 6.6 amperes, S24, with Skirted Edison Base, vertical coil tungsten filament, Type C lamp.

These filters were numbers 78, 78A, 78B and 78C. The matching with daylight was accomplished by means of a special mechanism devised for this particular purpose.

In this investigation all the spectral lights, except violet, were studied at 592, 148, 37 and 9.2 photons. The eye was exposed to the stimulus for 24 sec. and the after-image was projected upon white reacting fields of 592, 148, 37, 9.2, 4 and 0 photons. The *O* was required to note particularly the first phase of the after-image. Thus a constancy in attention was assured. Other after-images were observed but they were not recorded unless they were sharply distinguished from the first phase. The spectral lights used as stimuli were: 650, 580, 528, 458 and 428mμ.

Results

The significant results pertain to hue only. In respect to the other attributes of visual sensation, it may be said, on the basis of these experiments, that in general the brightness of the after-image depends directly upon the brightness of the field upon which it is projected. There evidently is a mixture between the brightnesses in the two cases. With increased brightness of the reacting field there is desaturation of the after-image. The most saturated after-sensations occur with medium values of the background.

The data for hue are shown in the tables. Tables I–V are for spectral lights of 650, 580, 528, 458 and 428mμ respectively. Each column gives results for a brightness, expressed in photons, of the spectral light for a given series. The rows of the tables show the brightness levels of the reacting fields. The tables, therefore, read as follows. Column 1 of Table I shows, for spectral light of 650mμ at 592 photons, the results of projecting the after-image of this light on the various backgrounds of 592, 148, 37, 9.2, 4 and 0 photons.

TABLE I

Showing Results when Stimulus of 650 mμ was Used

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	B Bg	G Gb	Gb	*
148	B Bg	Gb	Gb	
37	Bg B Bg	Gb B G	Gb G	Bg
9.2	Bv B Bg	Bg B	G Gb B	Bg B
4	Bv B	Bg B Bg	Gb	Bg B
0	V	Bg	Gb	Bg

*On bright reacting fields with after-images of weak lights there is merely a slight discoloration of the background. I am reluctant to call this a typical after-image.

Column 2 gives similar results for the same color, at the brilliance of 148 photons. When the after-image is a succession of colors, a series of color-symbols is given as each item of the table. Such a succession was always gradual or involved a blending of one color into another, for there were no colorless gaps. The letters in the tables stand for the following colors: R = red, O = orange, Y = yellow, G = green, B = blue, V = violet, and P = purple. These letters are often qualified by small letters which stand for colors of less dominance in the blend than the color indicated by the large letters. Thus Bg means a blue-green in which blue is dominant.

TABLE II

SHOWING RESULTS WHEN STIMULUS OF 580 mμ WAS USED

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	Pb	B	Bg	
148	P	B	Gb	
37	Pr Pb	Pb B	Gb G	G
9.2	Pr B P	Pb Pr	Bg	G
4	Pr B P	Pb B P	B	Gb G
0	Pr	Pb	V	B

TABLE III

SHOWING RESULTS WHEN STIMULUS OF 528 mμ WAS USED

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	Pr	Pr R	Pr	
148	Pr R	Pr O	Pr O	
37	Pr	Pr	Pr	Or
9.2	Pb P	Pr	Pr	OR Oy
4	Vb V	Pb	Pr	Or R Pr
0	Vb	Vb	Pr	Pr

TABLE IV

SHOWING RESULTS WHEN STIMULUS OF 458 mμ WAS USED

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592	148	37	9.2
592	O Yg Y	Or O Yg	Oy	
148	O Yg Y	Or O Yg	Oy O	
37	R O Y	O Yg	Y Yg	Yg
9.2	Pr O Y	Or Yg	O Yg	Yg
4	Pr O Y	Or Yg	O Y	Yg
0	P Yg	Pr Yg	Yg	Yg

TABLE V

SHOWING RESULTS WHEN STIMULUS OF 428 mμ WAS USED

Brightness of Field of Projection in Photons	Brightness of Stimulus in Photons			
	592*	148	37	9.2
592		Oy Yg		
148		Oy	Yg	
37		O Gy Yg	Y Yg	Gy
9.2		Or Yg	Yg	G
4		Pr Oy Yg	Gy Yg	G Gy
0		Pr Pr	Gy	G

*The blank column at brightness level 592 photons, is due to the fact that violet of this brightness could not be obtained by the apparatus.

In order to understand the tables, the reader must have well in mind the complementary colors of the spectral lights investigated. The complementary for 650mμ is blue-green or 490mμ; for 580mμ, blue or 475mμ; for 528, purple; for 458, yellow or 572mμ; and for 428mμ, yellow-green or 567mμ.

Frequency of the complementary hue.

650mμ. The blue-green complementary always forms one phase of the after-image in all instances.[6]

580mμ. The blue complementary is represented in all instances except at the lowest intensity. With very weak stimulation the complementary color is not experienced.

528mμ. The purple complementary is always represented.

458mμ. The complementary hue is less pronounced at 9.2 photons than are the orange and yellows of higher intensities.

428mμ. The complementary yellow-green is usually found but at the higher intensities it occurs only as one phase in the after-image.

Hue and intensity of reacting fields.

650mμ. Hue changes are not very pronounced except at the brightness levels of 4 and 0 photons. At these points the blue component is usually strengthened, particularly when the stimulus is intense.

580mμ. At 592 and 148 photons the red component of the purple is enhanced with decreasing values of the white backgrounds. In case of 37 and 9.2 photons the blue component, in contrast to green, tends to be enhanced for lower intensities.

528mμ. The hues tend to become more bluish with decreasing brightnesses of the reacting fields except in the case of 37 photons. The more characteristic fact is that the red-purple is the invariable hue.

[6]The exact complementary does not seem to be experienced in after-image and contrast colors. Colors that produce white have been tested by the after-image and contrast methods and the complementary color is either too blue or too red in comparison with the complementary of the white light equation. Cf. H. D. Cook and F. M. Kunkle. The qualitative relation between complementary and contrast colors, *Psychol. Monog.*, (no. 96), 1916.

458mμ. At 592 photons the red component is enhanced at the lower intensities. At 148 and 37 photons there is but slight change and none at 9.2 photons.

428mμ. At 148 photons there is a change from orange to purple or from red to blue; at 37 photons, from yellow to green.

Hue and intensity of the stimulus.

650mμ. Comparing the columns in Table I it is at once apparent that at 592 photons after-images are quite bluish. All of the after-images are either blue-green, blue, or violet. At 148 photons the blue component is weaker but still quite pronounced, at 37 the green is dominant, and at 9.2 photons there is a reversal toward blue.

580mμ. At 592 photons the appearance of the red component is a striking fact. At 148 photons the red component becomes weaker and the blue is enhanced. At 37 photons the green component is brought out and is still further enhanced at 9.2 photons.

528mμ. There is less variation with intensity than in the other colors. At 592, 148, and 37 photons the hue series are essentially in terms of the complementary purple. At 9.2 photons, and to some extent at 37 photons, the red component is enhanced.

458mμ. The red component is strong at 592 photons, persists at 148, is very much weakened at 37 and is eliminated at 9.2 photons.

428mμ. At higher intensities the red and blue components are both present. They disappear at 37 photons, where yellow and green are pronounced. At 9.2 photons the yellow is weakened.

Conditions most favorable for the complementary after-image.

650mμ. At 37 photons.
580mμ. Between 148 and 37 photons.
528mμ. The complementary color is invariably experienced under all conditions except low brightnesses of the reacting field.
458mμ. At 37 photons.
428mμ. At 37 photons.

Strikingly uncomplementary after-images.

650mμ. The blue and violet at higher intensities.
580mμ. The purplish effects at higher intensities.
528mμ. Nothing unusual; the purple is here complementary.
458mμ. The purple after-images at 592 photons.
428mμ. The purple after-images at 148 photons.

These results show that the complementary color was experienced under some conditions for all the colors studied. It is, however, more specifically determined for some intensities of the stimulus than for others. It is most pronounced at the lower intensities. Here the after-images are confined in the limits of

variation or succession of hues. These variations approximate the complementary color. At higher intensities the characteristic after-image is in terms of red or blue, usually both.

The most significant results are the changes of hue that were found to be a function of the intensity of the stimulus. They may be summarized as follows.

650mμ. Blue or violet to green-*blue* to blue-*green* to green-*blue*.
580mμ. (Purple) Red to blue to blue-green to green.
528mμ. (Purple) Blue to red.
458mμ. (Purple) Blue to red to yellow to yellow-green.
428mμ. Red and blue to green-*yellow* to yellow-*green*.

The changes of hue as functions of the reacting field are less significant than for intensity of the stimulus. There is very little change except at the lowest photon values of 4 and 0. From 592 to 9.2 photons the changes are essentially in terms of components already present. It would seem that above 4 photons the reacting fields act as essentially white backgrounds; below 4 photons as dark backgrounds. Complementary colors are favored at the upper region and deviations toward red or blue are introduced at the lower level. The suggestion of Miles to distinguish 'after-images' proper,[7] which are solely the result of the continued activity of the visual tracts, from the 'projection after-image,' which is in large measure influenced by reflected light from the area upon which it is projected, is in keeping with these results.

The intensity of the stimulus and the background may be related in terms of a combination of factors. When the stimulus is intense and the reacting field is weak, the purplish colored after-images are favored; when the stimulus is of medium intensity and medium brightness, the complementary after-image is facilitated.

Discussion

The first question that arises is as to whether the hue of the stimulus changed at the various intensities so as to produce the different after-images that have been noted. The so-called Bezold-Brücke phenomenon describes the changes of the hue of a spectral light as a consequence of reduced intensity of the light radiations. The law is illustrated by Exner[8] as follows: "If the

[7] G. H. Miles, The formation of projected visual images by intermittent retinal stimulation, *Brit. J. Psychol.*, 7, 1915, 420–434; 8, 1915, 93–126.
[8] F. Exner, Ueber die Grundempfindungen im Young-Helmholtzchen Farbensystem, *Wiener Ber.*, III (2a), 1902, 857.

color tones in the spectrum arise in general from simultaneous but unequal excitation of the three fundamental sensations, such a color tone will alter when the objective intensity is reduced, and will approximate more and more closely the hue of the strongest of the fundamental sensations composing it, since the weaker ones will sink below the threshold. One can verify this deduction by gradually reducing the brightness of the spectrum. The transi-

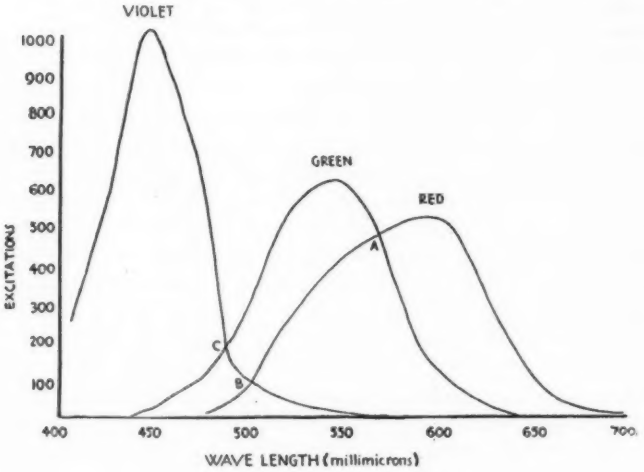

FIG. 2—EXCITATION CURVES

tions between red and green, and green and blue, and the violet all vanish, leaving only the three fundamentals or sensations approximating."

The application of this principle to the excitation curves of Fig. 2 is as follows: "'A' equals R and G—no B. The hue will be independent of brightness within wide limits; not so wave-lengths on either side of 'A'. A wave-length to the redwards of 'A' will become redder when the brightness is diminished, since the G will sink under the threshold. One to the greenwards of 'A' will become greener. 'B' equals G plus (equal red and blue). The hue will be independent of brightness. The wave-lengths on either side will be shifted in hue toward the yellow and blue respectively."

'C' is likewise an invariable and wave-lengths on either side of it will vary toward blue and green respectively.[9]

These curves do not explain the purplish after-images at high intensities. However, by making an assumption that is consistent with visual theory, the explanation can be considerably improved.

Suppose that we start from the low intensities and interpret the law in the reverse direction, since we are interested in explaining those after-effects of bright lights that are apparently incongruous with visual theory. Except at the invariable points, the dominant hue would be alone active at the very lowest intensities. With increasing intensities subordinate excitation-processes would tend also to increase. Suppose now that each excitation-process has a maximum or limit of functions, such that when all fundamental sensations are functioning maximally they combine to give white.[10] As soon as the dominant excitation-process has reached its maximum, further increase would tend to raise the subordinate excitation-processes while the dominant sensation remains relatively stable. Such a change would continue, gradually, until all the excitation-processes are functioning maximally and white would finally be experienced. One fact in support of this hypothesis is that all colors eventually become white if sufficiently bright.

Interpreting the observed results in accordance with these suppositions, we find that the blue-green at the lowest values of 650mμ is the complementary after-image which is typical for this stimulus. At a higher intensity the after-image is more bluish, a fact which means that the green excitation-process is being added to the red, causing it to be more yellowish. At the very highest intensity, the green process is still more pronounced and the red is more yellowish; and thus we can account for the richer blues and violets which were noted. In this case the blue excitation-process is not involved at all, as Fig. 2 shows.

[9]Exner actually used the curves of König and Dieterici in this description, but I have substituted in its place the excitation curves as determined by Weaver who averaged the data of König and Dieterici with those of Abney. The argument is equally true for those curves as for König and Dieterici's. Report of the Colorimetry Committee of the Optical Society of America, 1920–21, *J. Opt. Soc. Amer.*, 6, 1922, 548.

[10]H. von Helmholtz, *op. cit.*, II, 183.

Spectral yellow at 580mμ is not quite at the 'invariable point' but is placed rather toward the red. The red sensation is dominant and at low intensities this sensation is almost pure. Hence, we have the green after-image at low intensities. With higher luminosities, the green excitation-process is enhanced and the red and green sensation acts as if the eye were stimulated with an addition of yellow in the red, due to the mixture of the green and red processes. The blue-green after-image would thus be explained. At the highest intensities the red and green sensation would act as a yellow and the blue component in the after-image would be pronounced. This conclusion, therefore, falls short of accounting for the red component in the purple experienced at these levels. It seems that a greater addition of a green sensation is necessary.[11]

In the case of the green, we may explain the dominance of the red component at low intensities as due to the dominant green sensation. At the higher intensities the blue component would be increased with the addition of yellow caused by the mixture of red and green. Hence this accounts for the fact that the purples are more bluish at higher than at lower intensities.

Spectral light 458mμ may be similarly explained. At low intensities the complementary color or yellow-green is due to the relative purity of the violet excitation-process. At higher intensities the green process is more effective and tends to add red to the after-image. This red would be quite pronounced at high luminosities.

Spectral light 428mμ would behave similarly to 458mμ but the difficulty here is that the green excitation-process does not extend as far as this point.

This theory explains the results only in part. It explains some of the purplish effects at high intensities, and also the fact that for long wave-length of light the blue component is enhanced, while for the short it is the red component. This explanation is not, however, complete, for the purple is not adequately explained in the case of stimulus 580mμ nor the purple

[11]It is possible, however, to regard the purple, in this case, as a fusion of the after-images of the red and green excitation-processes. Ordinarily, it would be expected that the after-images are determined by the objective stimulus mixtures and that the red and green should determine after-images of a bluish kind.

for 428mμ, where the violet should be an invariable color (according to the diagram of excitation curves) because no other excitation-processes are involved. A similar situation exists at 650mμ, where apparently a pure red sensation yields after-images that indicate that it is quite mixed.

Much of the difficulty could be remedied if the green excitation curve were more extensive at the base, and the fact that purple results from the stimulating of yellow at high intensities indicates that probably these curves overlap much more than is apparent from the different descriptions of the excitation curves. The theoretical curves as originally formulated by Helmholtz overlap extensively and so more readily explain the results of this experiment than do the empirically determined curves.

One other matter that we have to mention is the fact that the after-image changes in hue at very low intensities of the reacting field. This finding may be explained, in part, by the fact that hues change with the admixture of black to them. Rood[12] showed that hues change when colored papers are mixed with black. Mayer[13] investigated contrast effects by observing 15 shades of gray on different colored papers. "Viewing in succession the gray disks on a violet surface, no. 1, 2, 3, and 4 (grays) appear yellow-green, no. 5 appears as much yellow as green, no. 6 apappears yellowish-green. The following disks in the order of their appearance appear greener and greener till no. 13 appears of a dark green almost devoid of yellow." Thus, it would seem that in this case the light gray backgrounds enhance the yellow component, while the dark gray papers enhance the green component in the contrast color. The same fact is also apparent in the after-images observed with stimulus 428mμ, and, in general, it may prove to be the explanation of change of hue at the lowest intensities.

Conclusion

The color of the after-image is specifically correlated with the brightness of the light simultaneously fatiguing the retina. Thus the after-image is complementary in hue only under particular

[12]O. N. Rood, *Modern Chromatics*, 1897, 197–201.

[13]A. M. Mayer, Note on the analysis of contrast color—by viewing through a reflecting tube a series of gray discs or rings on colored paper, *Amer. J. Sci.*, 4th ser., 1, 1895, 38–40.

circumstances. It is no more characteristic of the stimulus than is the purplish effect which is due to the same light under different intensities. A complementary hue is favored by medium intensity of the stimulus and reacting field; a purple hue is a consequence of the higher intensities. The curves of the excitation-processes indicatet hat the complementary effects are due to the dominant process, whereas the changes introduced by increased intensity of the stimulus may be due to the increased activity of the subordinate excitation process. However, the Bezold-Brücke phenomenon, which describes the hue of spectral lights as a result of change in their brightness, does not fully explain the hue of the after-image.

THE INFLUENCE OF REPETITION OF A SERIES UPON THE OMISSION OF ITS INTERMEDIATE TERMS

By Albert D. Freiberg, Karl M. Dallenbach, and Edward L. Thorndike

From time to time some student of behavior advocates or assumes the doctrine that the frequent occurrence of a series of physiological or psychological events (call it A→B→C→D→E) will tend to cause the omission of one or more of the intermediate terms so that in the end A→B→C→D→E tends to be replaced by A→E. Thus Jennings writes: "When a certain physiological state has been resolved, through the continued action of an external agent, or otherwise, into a second physiological state, this resolution becomes easier, so that in course of time it takes place quickly and spontaneously."[1] "The law may be expressed briefly as follows: The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times. Hence the behavior primarily characteristic for the second state comes to follow immediately upon the first state. The operations of this law are, of course, seen on a vast scale in higher organisms in the phenomena which we commonly call memory, association, habit formation and learning."[2] This law may be expressed conveniently as a tendency of a series of states $A \rightarrow B \rightarrow C \rightarrow D$ to become $A \rightarrow D$ or $A \rightarrow B' \rightarrow C' \rightarrow D$. B' and C' being states B and C passed rapidly and in a modified way so that they do not result in a reaction but are resolved directly into D.

Hollingworth writes, in his explanation of how false responses are eliminated and the correct response is selected, "with repetition . . . these intermediate acts drop out."[3] Hollingworth seems to attach a certain potency to the firstness of the first member of the series as well as to the repetition of the series. The end member, according to him, tends to be produced by that one of the preceding members "which first or unfailingly occurs."[4]

*Accepted for publication July 14, 1929.

[1] H. S. Jennings, *Behavior of the Lower Organisms*, 1906, 289.

[2] *Op. cit.*, 291.

[3] H. L. Hollingworth, *Psychology: Its Facts and Principles*, 1928, 214.

[4] *Op. cit.*, 216.

"The one which first and always occurs (hunger pang plus sight of food) will at once instigate the consequent (pulling the catch)."[5]

Thorndike has elsewhere discussed this doctrine in general.[6] The present note concerns detailed evidence bearing on it found by the examination of the mistakes made by the compositors of 1068 galleys of *The American Journal of Psychology*, comprising the years 1923-1927 inclusive. The action of a compositor in setting the type for a word, whether by hand, monotype machine, or linotype machine, is a fairly typical case of such a series of events. The sight of the word in the copy or the thought of it in memory is the first term or *A*, the movements of taking and setting the type from the proper compartments or pressing the proper keys are the *B*, *C*, *D*, etc., the last term of the series being the setting of the last letter.[7]

If frequency of occurrence of such a series tends to shorten the series by the omission of terms between the sight or thought of the word and the setting of its last letter, omissions of letters (1) should be much more common than additions, (2) should occur much oftener for intermediate letters than for final letters, and (3) should—other things being equal—be more common in words often set than in words rarely set.

The compositor would, for example, have strong tendencies to make *te* for *the*, and *tht* or *tat* for *that*. These would have to be counteracted by other forces; and if the compositor's control broke down at all so that he made mistakes of any sort other than those of ignorance, these tendencies should appear.

In the material which we have examined, there are 373 cases where a word has a letter or letters omitted, and 324 cases where a word has a letter repeated or a letter or letters added. The omissions do not come in intermediate positions more often than chance would prescribe, but very much less often. 16 of them are at the beginning of a word; 158 of them are at the end of a word; 209 of them are in intermediate positions. The average length of the words for which letters were omitted is approximately 9, so occurrences at the end of a word are, per oppor-

[5] *Op. cit.*, 214.

[6] E. L. Thorndike, *Animal Intelligence*, 1911, 266-272.

[7] Sometimes, of course, the series is only a part of a word; at other times it includes more than one word.

tunity, about 5 times as frequent as in intermediate positions. The omissions of intermediate letters do not occur very often in words which have been very frequently used. On the contrary, as the complete list below for 1923 and 1924 shows, they are more frequent per opportunity in rare words.

Words from which intermediate letters were omitted. Gedach(t)nis; tur(c)ica; deficienc(i)es; intense(l)y; sugge(s)tions; as(s)umptions; non-substan(t)ial; i(rr)educible; gro(s)grain; pos(i)tions; cond(i)tioned; suc(c)eeded; fulfil(l)ing; material(i)zations; qualit(at)ive; occur(r)ing; Marse(i)llaise; quanti(ta)tive; decide(d)ly; direct(ed)ness; indiffer(e)ntly; hyg(i)enic; sla(c)ken; occur(r)ence; withdraw(a)l; psy(c)hological; decide(d)ly; equal(iz)ing; zusammenges(e)tzten; critici(s)ms; Ges(t)alt; gen(e)tic; invol(v)ed; sy(s)tem; nervo(u)s; fulfil(l)s; i(n)asmuch; moral(i)ty; conscien(c)e; criti(ci)zed; abstra(c)tions; mor(t)ality; actu(a)rial; diffi(c)ult; implicit(l)y; explicit(l)y; indescri(b)able; intermitten(t)ly; embarras(s)ment; vari(a)ble; adapt(a)tion; len(g)ths; intermitti(e)render; co(n)genital; thre(s)holds; de l'(e)space; si(m)ilar; stre(n)gthen; jud(g)mental; W(a)hrnehmungswelt; Romant(i)ker; Yuj(e)ro; on(l)y; perserver(a)ting; Gestal(t)en; l(i)kely; caut(i)ous; differ(e)ntiating; for(e)see; Charlott(e)sville; rhy(th)mische; Mü(n)chen; b(a)sis; b(r)ought; visu(a)lization; psychi(c)al; Scho(e)nheinz; neces(s)arily; skel(e)ton; c(l)ose; pren(a)tal; appetit(i)ve; pleas(a)ntness; des(s)ert; p(i)ece; definite(l)y; l(i)ght; p(l)easantness; p(l)ane.

If we take the five hundred commonest words in English reading matter—plus also the plurals of the nouns included—which compositors of five years experience would have set from a thousand or so to fifty thousand or more times, we find the following.

Omissions at the beginning of a word	5
Omissions at the end of a word	34
Omissions in intermediate positions	16
Additions	36

On the whole, the facts give no support to the doctrine that the frequent repetition of a series of events in the mind or body in and of itself produces a tendency toward the omission of the intermediate terms of the series.

THE DETERMINANTS OF CONFIDENCE

By Arthur Jersild, Columbia University

The present study is addressed to the following questions. To what extent is confidence a correlate of real skill? To what extent may a display of self-assurance be regarded as a character trait relatively independent of actual competence? What is the relation between confidence and correctness?

The experiments here reported were conducted in connection with classroom examinations. True-false tests, covering reading assignments and lectures, were administered. The plan was to obtain (1) a measure of the *Ss*' actual information; and (2) a measure of the degree of certainty or confidence which they attached to their answers.

The *Ss* were told to mark every proposition either + or − (meaning true or false). The number of propositions correctly marked as true or false constituted, accordingly, their 'Information Score.' They were further directed to indicate the degree of confidence which they felt in their judgments by using double marks (as + + or − −) when they were absolutely certain of their answers, by using single marks (as + or −) when they were only fairly certain, and by querying their marks (as +? or −?) when they were guessing. The *Ss* were led to believe that these confidence markings would be used in the computation of their grades, and that the usual formula for correcting true-false tests (number right minus the number wrong) would be applied.

For the purpose of this experiment values of 3, 2, and 1 were assigned to the three respective degrees of confidence. The total count of all these values on a given paper constituted the 'Confidence Score.' This score was computed irrespective of signs and correctness.

The above procedure was followed with two classes;[1] one of these, numbering 53 students, consisted of sophomores and upperclassmen; the other, with a membership of 32, enrolled graduate and undergraduate students. Two tests were given to each group.

Results. The method of correlation applied to the results gave the findings indicated in Table I. This table gives the coefficients of correlation between the measures of Information and Confidence. A and B designate the two respective tests given to each of the two groups. The P.E.s are given in parentheses.[2]

The first two entries in the table above show a positive correlation bteween confidence and information. It appears that actual knowledge is one determinant of a confident attitude. But the relatively low magnitude of the coefficients indicates that in individual cases high confidence may go with low performance, and vice versa.

Of greater interest is the fourth entry in the table, which shows the correlation between confidence as displayed on the two test occasions. The coeffi-

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[1]These classes were made available for this experiment through the kindness of Professor H. L. Hollingworth.

[2]The Product-Moments correlation method was used with Group 1; the Rank-Difference method with Group 2.

TABLE I

Factors correlated	Test	Group 1 (n = 53)	Group 2 (n = 32)
Grade and Confidence	A	0.44 (.08)	0.09 (.13)
Grade and Confidence	B	0.39 (.08)	0.31 (.11)
Grade	A and B	0.62 (.06)	0.58 (.08)
Confidence	A and B	0.75 (.04)	0.79 (.05)

cients shown for the two groups, 0.75 and 0.79 respectively, are decidedly higher than the values indicating the correlation between information and confidence, on the one hand, and between information scores obtained on the two tests. Of the two factors here involved, confidence and achievement, the former is the more constant. It appears that a show of confidence is to a large degree a matter of personal idiosyncrasy and that it cannot be taken as a reliable gauge of the actual merit of the performer.[3]

There remains the further question as to the relationship between confidence and correctness when the judgments of the group as a whole are taken into account. The data of the class of 53 members were examined with a view to answering the question. A count was made (1) of the total number of entries shown for each of the three degrees of confidence; and (2) of the number of errors shown in each of these categories. From this could be calculated the percentage of correct answers accompanying each of the three degrees of confidence. This procedure gave results as follows.

Degree of confidence	*Percentage of correct answers*
Absolutely certain	89
Fairly certain	71
Guess	61

These determinations show that a report of 'absolute certainty' falls a good deal short of indicating perfect accuracy; and that a 'guess' has more than a 50% chance of being correct.[4]

[3]In an extended study, W. C. Trow (The psychology of confidence, *Arch. Psychol.*, 1923, no. 67, 1-47) found practically a zero correlation between the total achievement and confidence scores on a wide variety of tests ($r = -0.03$). He found also that the range of every *S*'s display of confidence in different situations was so great that no single test would, in his opinion, serve as an adequate indicator of a general confidence trait. In view of Trow's results, the findings in the present study, namely, that there is a reliable positive correlation between confidence and achievement, and that the *S*'s confidence will be a more constant factor than his achievement in going from one performance to another within the same type of test situation should not be given too general an interpretation.

[4]The relationship between confidence and correctness has been studied under a variety of conditions by previous investigators. See H. L. Hollingworth, Experimental studies in judgment, *Arch. Psychol.*, (no. 29), 1913, 1-119; V. A. C. Henmon, The relation of the time of a judgment to its accuracy, *Psychol. Rev.*, 18, 1911, 186-201; and Trow, *op. cit.* In each of these studies four degrees of confidence were indicated, so the results are not directly comparable with the above. The figure shown above for the highest degree of confidence (absolute certainty) closely resembles, however, the results reported by Henmon and Trow, though it is somewhat lower than the determinations found by Hollingworth. There is a close agreement between the various reports on the percentage of correctness found in the lowest degree of confidence.

Of some incidental interest in this connection is the question as to whether greater confidence attaches to an affirmation (judgment that a proposition is true) than to a denial (judgment that a proposition is false). Examination of the data revealed no statistically reliable weighting of confidence in either direction. Similarly, it might be asked whether greater accuracy attaches to an affirmation than to a denial. This problem was met by finding the percentage of correctness for all the true and all the false judgments. The results showed that 86% of all the propositions marked "+" were correctly marked; while 83% of those marked "−" were correctly marked. There is a difference of 3% in favor of the affirmations. This difference is too slight to warrant extended comment. The direction of the difference would indicate that there is a greater tendency to mark as false statements which are actually true than to mark as true statements which are false. In other words, there is greater accuracy in rejecting false propositions than in evaluating true ones. Evidence of a more distinctive order on this tendency was found by Strong in a study with advertising materials;[5] it appeared from his results that "the ability to know we have *not* seen an object is much more strongly fixed than the ability to pick out what we have seen."[6]

The main findings of the present study have been indicated above. It appears first of all that actual information determines confidence to some degree. There is a low but positive correlation between confidence and information scores and in the group results the higher the degree of confidence reported, the higher is the percentage of accuracy. But it appears also that, so far as the individual is concerned, the display of confidence is determined to a larger extent by individual caprice than by demonstrable quality of workmanship. There is a greater correspondence between the confidence shown in each of two situations than between the merit of the performance in the two instances, and in the separate situations the correlation between confidence and correctness will be low.

This latter result is amply verified by common observation. It is not unusual to encounter a person who displays utter self-assurance without showing commensurate skill, and the opposed type of individual whose show of confidence is relatively low while his demonstrated competence is high. Two factors, one may suppose, are responsible for the display of confidence which appears independently of real achievement. On the one hand, the individual may assume a confident attitude as a 'front' to compensate his recognized inadequacy. On the other hand, and this account seems more probable, the incompetence which an individual reveals in the poor quality of his performance may be associated with a corresponding lack of insight into his own deficiencies. This same principle may be urged to account for the phenomenon of high achievement associated with a relatively low display of confidence. The person of superior attainments will be able to recognize more fully that his grasp on things, however adequate to the occasion, still falls far short of thorough versatility. But the proof of this tenuous hypothesis must await further investigation.

[5] E. K. Strong, The effect of length of series upon recognition memory, *Psychol. Rev.*, 19, 1912, 447-462. Quotation from page 462.

[6] *Ibid.*, 462 (italics mine).

AN ANOMALY OF FOVEAL COLOR PERCEPTION

By Ellis Freeman, University of Louisville

It is customary in dealing with visual perception to adopt the anatomical-physiological attitude and to suppose in general that the visual experience corresponds to the retinal excitation. Particularly in the study of visual acuity has it been assumed, without the suspicion of other possibilities, that if in two cases the ultimate retinal images be identical, the resulting experiences, that is the resolution of the complex (or, as it is sometimes called, the perception of form) will be identical regardless of the size and distance of the stimuli. This means that, under identical illumination, a large distant figure would be resolved as well as a similar small and correspondingly near figure. More broadly speaking, the view that has represented physiology, and to a large degree psychology, has been that visual acuity is accounted for by the laws of geometrical optics, that intensity of illumination and contrast and visual angle alone determined the experience of resolution of a figure or letter. In papers already published it has been shown that both foveal and peripheral acuity deviate from the inferences of geometrical optics to an astonishing degree,[1] when large distant figures and small near figures are employed, although both give identical conditions of retinal stimulation. Experiments of this nature, in addition to supplying new data, have considerable theoretical importance inasmuch as they prove the anatomical-physiological viewpoint inadequate, and show the inferences of geometrical optics, which declare that where there are identical retinal images there are identical visual perceptions, not to be universally applicable.

The present article is part of a study of the relation between visual stimuli and visual experience. Our problem here is specifically to discover whether the same anomalies occur in color perception as in the perception of form and in visual acuity. In the papers mentioned above the anomalies associated with visual acuity have been fully described. It is necessary, however, in order to show what problem these anomalies suggest for color vision, to mention them again.

Jaensch,[2] in an experiment on visual perception, obtained the following results. When the letter E was used in three sizes corresponding to the proportions 1:1.57:2.45, they were not recognized at distances which varied in the same proportion but at distances whose ratios were 1:1.123:1.496. The larger letters required a greater visual angle for resolution than the smaller. This

*Accepted for publication August 6, 1929.

[1]Ellis Freeman, What does a test of visual acuity measure? *Arch. Ophth.*, 2, 1929, 48-56; The anomalies of peripheral visual acuity, *J. Exper. Psychol.*, 12, 1929, 324-340.

[2]E. R. Jaensch, Zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol., Erg. Bd.*, 4, 1909, 60-67.

result is a direct contradiction of the laws of geometrical optics. Jacobsson,[3] employing a more precise method that involved a tachistoscopic exposure of letters and geometrical figures which varied in size and distance (which was proportionate to their size), secured results similar to those obtained by Jaensch. The larger stimulus-objects were recognized correctly less often than the smaller. Von Lempicka,[4] working on the problem of color fusion, obtained similar results. She used, as her stimulus-objects, disks of four different sizes which were cross-marked with black and white bands. The width of the cross bands varied proportionally with the areas of the disks. The disks were at first placed at distances too great for her *Os* to distinguish the bands. Then the *Os* approached the disks until they reached the point at which the bands could be clearly distinguished. The results show that the large disks suffer a handicap in that they must be brought for resolution to a larger visual angle than that required by the small.

The work of Guillery[5] on the smallest retinal image necessary for the perception of color supplies a transition from the investigations concerned only with acuity to the subject of this paper. Just as other writers supposed that they were giving a constant value for acuity,[6] to which only a coefficient for distance had to be applied, so Guillery supposed that he had determined a physical constant for color sensitivity in terms of required area of retinal stimulation. His results are that at a distance of 1 m. from the eye the following diameters of the retina must be stimulated in order to produce color perception: blue, 0.019 mm.; green, 0.011 mm.; yellow, 0.007 mm.; red, 0.007 mm. The noteworthy fact is that in giving these values, Guillery thought that he was giving constants that in principle were final. Nor did it occur to him that merely with a change in distance the area of retinal stimulation might also have to be changed before the color perception could be produced.

The problem as it shaped itself for our work was, then, if a given square of color be barely perceived at a given distance, at what distance will a larger square of the same color, under the same illumination, be perceived? Will the larger be recognized at a distance proportional to its size in accordance with the laws of geometrical optics; or will it be recognized at a lesser distance creating a situation similar to that of the work mentioned here on acuity? Or, as is conceivable, will the larger be perceived at a distance even greater than one proportional to its size?

The experimental procedure was as follows: a piece of colored paper 5 mm. sq., pasted at the center of a gray cardboard 60 mm. sq., was brought toward the *O* until he just barely recognized its color. This was repeated 4 times and the average distance of recognition was computed for the 4 trials. Then the procedure was repeated with color-areas and exposure-cards that were enlarged

[3] Malte Jacobsson, Über die Erkennbarkeit optischer Figuren bei gleichem Netzhautbild und verschiedener scheinbarer Grösse, *Zsch. f. Psychol.*, 77, 1917, 1-91.

[4] Wanda von Lempicka, Räumliche Farbenmischung auf der Netzhaut, *Zsch. f. Sinnesphysiol.*, 50, 1919, 217-251.

[5] —— Guillery, Vergleichende Untersuchungen über Raum-, Licht- und Farbensinn in Zentrum und Peripherie der Netzhaut, *Zsch. f. Psychol.*, 12, 1896, 243-274; especially 267-270.

[6] G. E. de Schweinitz, *Diseases of the Eye*, 1924, 34–36, 68–69; J. H. Parsons, *Diseases of the Eye*, 1927, 127-136.

10 diameters, *i.e.* the colored papers were 5 cm. sq. and the cardboards were 60 cm. sq. The experiments were conducted under an even gray sky on six different days with 10 *Os*. In some of the experiments the large cards were exposed first, in others the small cards were exposed first, consequently the effects of practice were equalized. The colors used were red, blue, green, yellow, and orange.

Theoretically, if one were to base his reasoning on geometrical optics as do the physiologists and ophthalmologists in discussing acuity, it would be necessary to infer that, as the intensity of illumination, the color quality, and the background gray were constant between any one small square and its corresponding large square, the only difference would lie in the fact that the large was perceived 10 times further away than the small, since it was 10 times larger. This notion is based on the generally unquestioned conviction that where all things else are equal the only determinant in question is the visual angle—the size of the retinal image—regardless of the distance of the object which produces it. The experimental work bears this out no more than the work already cited on acuity bears out a similar assumption with regard to the resolution of letters and figures.

The following summary is given of the data of one *O*. *Red:* small square recognized at 21 m., large square at 160 m., giving a ratio of 1:7.6; *blue:* small square recognized at 10 m., large square at 50 m., ratio 1:5; *green:* small square recognized at 9 m., large at 30 m., ratio 1:3.3; *yellow:* small square recognized at 28 m., large at 190 m., ratio 1:7; *orange:* small square recognized at 21 m., large at 150 m., giving a ratio of 1:7. Similar results were obtained from the other *Os*.

For the present these facts can only be presented without comment. As far as the writer knows nothing is available to explain the anomaly existing between the perception of the large and the perception of the small squares of color.

APPARATUS

A Silencer for Laboratory Keys

By S. M. Newhall, Yale University

The silencer is merely a quiet contact which can be substituted for the regular lower contact of an ordinary electrical key. Fig. 1 shows a key with the original contact removed and the special unit bolted in the old hole at (A). It is best adapted to keys of this unpivoted spring lever type not requiring a stop to limit the lever movement on release. Keys like the one pictured do have a stop but it may be screwed up out of the way and not used. Keys of the pivoted lever type necessitate silencing of the impact at the stop when the key is released. A thin rubber cushion such as a rubber band is effective and it may be placed either on the stop itself or on the lever. With either type there must be usually some readjustment of the separation of the contacts after installing the silencer, but that is very simply achieved by turning the adjustment screw carrying the upper contact.

Design and materials take account of two causes of noise, namely, impact on the make and arcing on the break. The electrical contact is of platinum attached to the center of a spring brass strip. The latter is bent to a rectangular form and the two ends fastened to the flattened head of a small bolt as shown at (B). The platinum is thin to limit the jar of impact at the moment of contact. The brass is also thin and it is cut with the grain so that it will yield for the same reason. It has large surface relative to thickness to give lower vibration rate since that tends to raise the intensive threshold. The frame construction helps to avoid slow, ragged break from excessive yielding, and a fairly light pressure in operating the key is desirable for the same reason. Adjustment of the contacts to small clearance when open favors light pressure and slight impact. While the yielding of the lower contact is disadvantageous in that it may increase the latency or variability of the break, there is, fortunately, little or no such effect on the make. The function of the rubber cushion contained within the brass frame is to absorb the vibration of impact. Any slight rattle that may be noticed is probably due to a loose binding screw or lock-nut and of course can be corrected by tightening.

Constructional details which have proven effective are given below. The spring strip is 60 mm. x 5 mm. cut from 33 B & S gage stock. This gives a cross section of over 2000 cir. mils which is sufficient to eliminate difficulty from heating. The strip is bent transversely and at right-angles in four places to form the closed rectangle, 25 mm. x 5 mm., with the ends meeting in the middle of one of the longer sides. Obviously, two of the bends are at 12.5 mm. from the respective ends of the strip and the other two at 17.5 mm. A round-headed 10 x 32 brass machine screw, cut to suitable length, serves as the attachment bolt. I used this size because it fitted the hole from which the original contact was removed. The head of the screw is filed down to about 1 mm. thickness and to the flat surface thus formed the free ends of the rec-

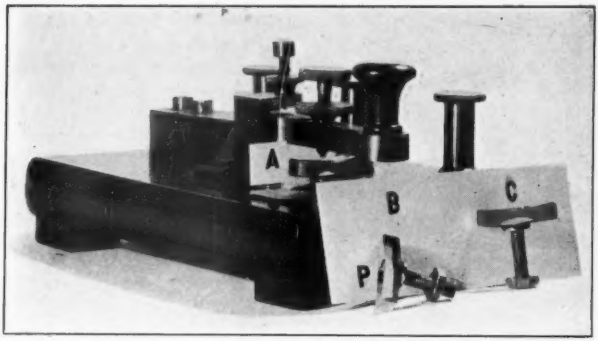

FIG. 1. LABORATORY KEY WITH SILENCER

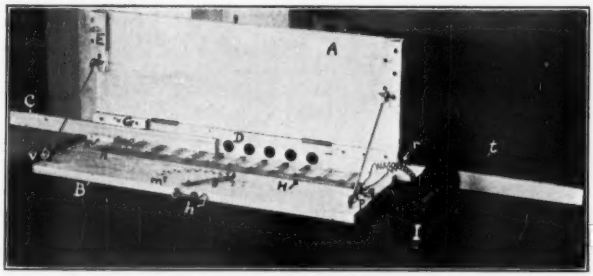

FIG. 2. THE MULTIPLE-CHOICE APPARATUS
(Seen from the operator's side)

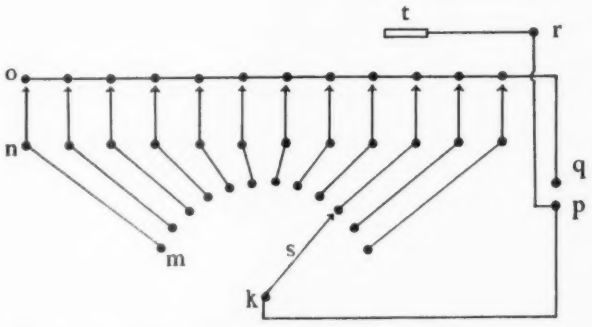

FIG. 3. WIRING DIAGRAM OF THE MULTIPLE-CHOICE APPARATUS

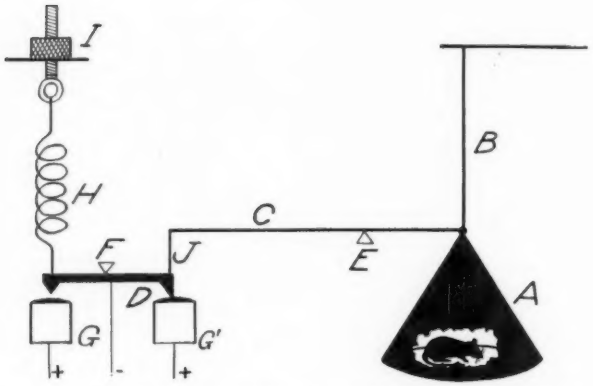

FIG. 4. DIAGRAM OF CONTACT AND SUSPENSION DEVICE

A, Suspension basket; B, rubber band; C, lever, 34 cm. long; D, lever, 5 cm. long; E and F, fulcrums; G, G′ mercury contact cups; H, small coil spring; I, screw for adjusting coil spring; J, strong thread about 2 cm. long joining the two levers. A slight downward push at A causes the electrical circuit to be closed at G, or broken at G′.

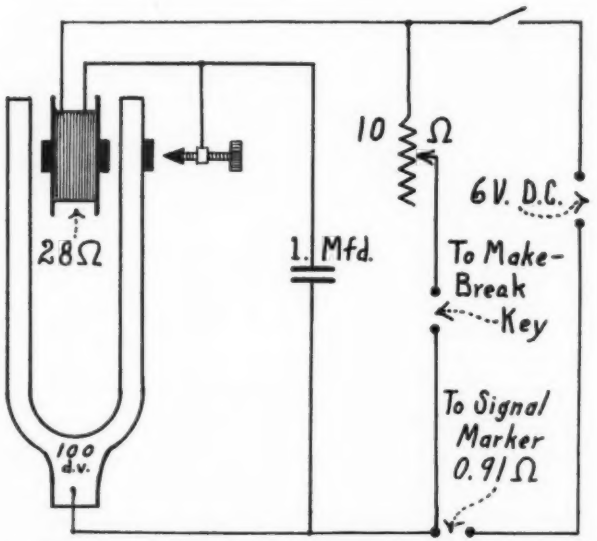

FIG. 5. WIRING DETAIL FOR THE TRIPLE-DUTY SIGNAL MARKER CONTROL

tangular frame are securely soldered. For electrical contact, a piece of sheet platinum about 5 mm. square is sweated to the long side of the frame in line with the axis of the bolt, as indicated at (P). The cushion is cut from a Goodyear size 30 red rubber eraser, 10 mm. wide and of such length and thickness as to fit snugly but not tightly into the frame. An arch-shaped portion about 13 mm. long is cut away from the under side of the cushion to increase its resiliency. This is shown at (A) or (C). The action of the frame is improved by bending the upper side to a slight concavity and the lower side to a slight convexity as shown at (A), (B), or (C).

The silencer is adapted to laboratory circuits of small inductance and current such as commonly used to operate a relay or electromagnetic recorder. When the silenced key was tested for audibility by making or breaking the circuit through the coils of a 20 ohm or 175 ohm relay, the results were satisfactory even with more than enough current to give positive operation of the instruments. Only a few observations were made but the observers knew what kind of sound to expect, when to expect it, the ear was normal to the source, and the room was fairly quiet. Under the conditions the limit of audibility seemed to be of the order of 50 cm. when the key was operated without special care and 10 cm. or less when the manipulation was rather slow. Though a slight spark could be seen in the dark room, under no circumstances was it aurally noticeable. Therefore, if some circuit should produce an objectionable arc at the key contacts, that could be eliminated by opening or closing the circuit through a relay so placed or mounted as to be inaudible to the observer. That method has the advantage of its certainty over the simpler one of shunting a condenser directly around the key. The quiet contact described has the advantages of simplicity in design, ease of installation, and freedom from mercury or other fluid.

A Multiple-Choice Apparatus

By K. E. Zener, Duke University, and E. G. Wever, Princeton University

The Multiple-Choice Method, developed by Yerkes from a type of problem which Hamilton devised for the testing of mental defectives,[1] has proved most valuable in the study of behavior of the problem-solving sort. Briefly stated, this method involves the presentation to the subject of a number of mechanisms, one or more of which are to be operated for the solution of a given problem; the problem consists in the discovery of the proper mechanism to operate in successive trials. Typical problems may require response to the right-hand member of the series of mechanisms exposed, or the left-hand member, or the middle member, etc.

The method has perhaps been most extensively used in the investigation of problem-solving in animals, though some application has been made to human adults and children, both normal and defective. The usefulness of the method in the investigation of normal human adult responses has been signally en-

[1] R. M. Yerkes, A new method for studying ideational behavior, *J. Comp. Psychol.*, 1, 1921, 369-394.

hanced by the work of Brown and Whittell, who have developed a graded series of problems to be set before the subject in order of difficulty.[2] These investigators thus have elaborated a valuable instrument for research purposes. Incidentally they have also afforded us an experiment suitable for elementary laboratory classes in psychology, and an experiment that is as interesting as it is instructive.

In giving the experiment to a laboratory section of any size there of course arises the serious problem of equipment; this problem the writers have attempted to solve by designing a model of multiple-choice apparatus that can be made up in quantity at a reasonable cost.

Description of the apparatus. The apparatus is shown, from the *E*'s side, in Fig. 2. *S* sits behind the apparatus, holding in one hand the brass electrode (t). His view of *E*'s manipulations is obscured by the board (A). The holders (E) are provided for the insertion of a sheet of heavy cardboard for more complete screening than the board A affords, which may be found necessary if unruly *S*s such as children or abnormals are being tested; normally the cardboard will not be needed. Before *S*, in the board (D), are twelve 1-in. holes, any number of which may be exposed at one time. The two wooden sliders (C) are provided for varying the number of holes exposed. A finger extended through one of the holes will operate a key, which is simply a brass strip (n) mounted above a screw-head (o) so as to touch it when depressed. Pressing a key completes an electric circuit and gives *S* a shock in the operating finger, unless the response is the 'correct' one, in which case a short-circuit of the current will have been provided by a setting of the lever (s). This lever may be set so as to make contact with any one of twelve screw-heads (m), and thus short-circuit any one of the keys.

An understanding of the electrical connections will be facilitated by an examination of the wiring diagram in Fig. 3. Most of the wiring runs beneath the board (B), and is concealed from view. From (q), which connects with one side of the electrical supply, a wire runs to each of the screws (o) with which the keys make contact. From (p), which connects with the other side of the electrical supply, one wire runs to the electrode (t), and another to the screw (k) on which the lever (s) is pivoted. The screws (m) that are arranged in the arc of the lever's swing are each connected with a corresponding key. When the lever is turned to a given screw, it is plain that pressing the key to which it corresponds will give no shock, since the current will take the path of lower resistance. Pressing any other key, on the other hand, will lead to punishment in the form of shock.

This instrument is simple enough in operation so that the beginning student has no trouble with it and rarely makes a wrong setting. The screw-heads (m) are plainly numbered from 1 to 12 to correspond with the keys, and every *E* is supplied with a direction-sheet containing the problems and exact settings to be made. We have followed Brown and Whittell's list of settings.[3]

The apparatus has been made to fold up when not in use, and can be stored in a small space—an important consideration in most laboratories.

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[2]Warner Brown and F. Whittell, Yerkes' multiple choice method with human adults, *ibid.*, 3, 1923, 305-318; esp. 311-2.

[3]*Op. cit.*, 311-2.

An Apparatus for Measuring the Reaction-Time of White Rats to Noise

By Martin F. Fritz, Iowa State College

Every one working with rats has observed that they jump quickly whenever sudden noises occur. In order to measure the reaction-time of this movement the author devised the apparatus shown in Fig. 4.

The experimental animal is placed in basket (A)—an aluminum disk suspended by three wires and covered with black cloth—which is supported by a rubber band (B). The strength and length of the rubber band is varied according to the weight of the rat so as to bring the lever systems (C) and (D), to which the basket is also attached, into balance. Lever (C) is connected to lever (D) by a string (J). String (J) is kept taut by the action of spring (H), the tension of which is regulated by a set screw (I). Lever (F) acts as a double switch, making a circuit at mercury cup (G) and breaking a circuit at mercury cup (G'). The apparatus may consequently be used with any of the standard chronoscopes or chronographs with either a 'make' or a 'break' circuit.

The stimulus, which should be a sudden sharp noise of short duration, can be given by means of the Wundt sound-hammer, or by any apparatus that will produce a noise and at the same time make or break an electrical circuit. The author used a mouse trap adapted to make an electrical circuit when it snapped.

The reaction-apparatus can be adjusted by means of the rubber bands (B) and the set screw (I) for the registration of very slight movements. For example, in the apparatus that the author used, a downward pull on the basket (A) of 1.9 grm., which moved it through a distance of approximately 2 mm., was sufficient to make or break the electrical circuit.

The author believes that this apparatus, besides measuring the rat's reaction-time, will prove useful in testing the effects of drugs and diets, of training and fatigue, etc. He feels, from his experience with it, that it will measure more delicately and more accurately the influence of these factors upon the nervous system and organic condition of the animal than either the maze or the problem box.

A Triple-Duty Single Magnetic Marker

By Samuel Renshaw, Ohio State University

Fig. 5 shows a wiring diagram for a single magnetic marker, fork-controlled, which does triple duty. It indicates the time of make and break and records in hundredths of a second the interval between these points. In our laboratory we have found the device useful for the chronographic measurement of reaction-times, for timing tachistoscopic exposures, etc. The marker indicates without appreciable lag the instant of make, the duration of the closing of the circuit in hundredths of a second, and the instant of the break on a single line on a kymograph. The magnet of the marker is arranged so that it is oscillating continuously at a very low amplitude. The make consists simply in stepping up the current to this marker by by-passing it around a variable resistance.

The 100 d.v. fork oscillates continuously, driven from a 6-volt D.C. source. The coil activating the fork has a resistance, in our instrument, of 28 ohms. The fork contact is of the bridge type, and is patterned after the one used by the Stoelting Co. in their control forks for the Dunlap chronoscope. A one m.f. fixed condenser is shunted about the breaker point on the fork to eliminate arcing. The small magnetic marker which we use is of the type made by Ebersbach of Ann Arbor, Michigan, and is wound to a resistance of 0.91 ohms. This marker is fitted with a small spring-steel armature of high period. The variable resistance of 10 ohms is adjusted by the experimenter so as to dampen appropriately the amplitude of the marker while the fork is in constant oscillation and before the circuit is made; for example, by the depression of the experimenter's key in a reaction-time experiment. Closing this circuit sends more current to the marker, thus increasing its amplitude; and breaking the circuit by the reactor's key restores the marker to its original status of oscillating at very low amplitude.

We have mounted these units on an oak board 12 by 12 inches. They have given us highly satisfactory results in the uses indicated above.

A somewhat similar device was described by Titchener.[1] The present device is a modification and improvement of the same principle originally used by W. G. Smith.[2] The points of difference may be observed by comparing the diagram of Smith's hook-up with that of the above figure.

Some New Auditory Apparatus

By Forrest L. Dimmick, Hobart College

During the past year the author has been using in the Hobart laboratory an instrument—the wiring diagram of which is shown in Fig. 6—for the production of relatively pure tones that covers a wider range than the full set of Stern variators, by the manipulation of at most two controls (a coarse and a fine adjustment) and that provides for a complete gradation of intensities from zero to maximum. The instrument consists essentially of two vacuum tube oscillating circuits one of which is fixed to oscillate at about 60 kilo-cycles while the other is variable between the approximate limits of 50 and 60 kilo-cycles. The beat-frequency between the two oscillating circuits is picked up and put through a detector and then an amplifier stage, thus emerging as a wave of voltage intensity whose rate depends upon the difference in rate of the two oscillating circuits. Any loud speaker whose leads are attached to the output terminals gives the corresponding tones. With a one-half turn of the dial one can go from the lowest audible tone to one near the upper limit. The manufacturers give the limits as approximately 20 to 9,000 cycles. The two instruments in the Hobart laboratory both exceed this range, especially at the lower end of the scale. Steady pulsations as slow as 4 to 5 per sec. can be heard with

[1] E. B. Titchener, *Experimental Psychology, Quantitative Experiments, Instructor's Manual*, 1905, 338 f.
[2] W. G. Smith, *Proc. Physiol. Soc.*, Nov. 17, 1894.

a telephone head set, though they are too weak to be audible on the loud speaker. Thus the lower limit seems to be set by the capacity of the speaker.

According to the manufacturers the form of the output wave is very nearly sinusoidal, "the total of harmonics being at a maximum but 4% of the wave in voltage." We have found this to be approximately true with both of our instruments below a tone of about 5000 vs. Above that some harmonics are audible. We have not yet attempted to determine the reason for this or to

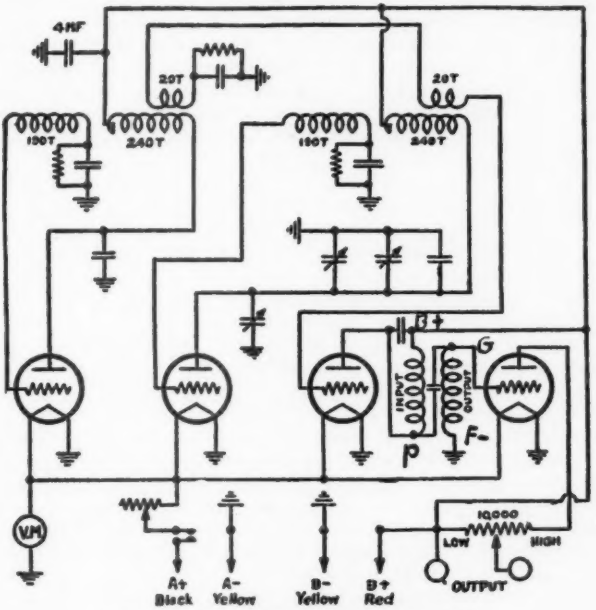

FIG. 6. BEAT-FREQUENCY OSCILLATOR
(Type 413, General Radio Co.)

eliminate it. Variations of intensity are obtained by means of a potentiometer which is bridged across the output. A turn of about 300 degrees gives the full range of intensities available. One of our instruments is the commercial model put out by the General Radio Company. The other we were able to build in our own laboratory thanks to the courtesy of the manufacturers who furnished the necessary specifications. Both instruments are equally satisfactory. The commercial model is designed to use either dry cells or external batteries. Equipped with the former, it is completely self-contained except for a loud speaker and fully portable. It is, therefore, excellent for lecture demonstrations. If the instrument is to be used for research purposes, addi-

tional equipment may be required to suit the particular experiment. For example, we have found it necessary to add a graduated dial to the intensity adjustment, and to provide a special switch in the leads to the speaker. For the latter any one of the better grades of instruments will serve. The author uses a 560 AW Westinghouse cone.

In order to turn the current on and off into the loud speaker, we have resorted to the following device. The output from the oscillator is carried first to a General Radio speaker filter number 387 A, and the output from this is bridged by a 10,000 ohm potentiometer. The speaker leads are connected to one side of the filter output and to the moving member of the potentiometer. By means of a small gear on the stem of the potentiometer which meshes with a larger geared quadrant on a lever arm the intensity of the output into the speaker is raised from zero to maximum in a fraction of a second. This eliminates the click in the speaker that occurs when an ordinary contact is made or broken.

Additional Improvements of the Sanford Chronoscope

By Christian A. Ruckmick, University of Iowa

Dr. Dimmick's further improvement of the Sanford chronoscope is decidedly a step in the right direction.[1] The substitution of the metal rods for the troublesome threads in the older forms might easily be combined with the magnetic release suggested by me in a previous note.[2]

I should like, however, to suggest three additional modifications which appear to make the apparatus still more dependable and convenient. (1) The main hexagonal adjusting-nut should be replaced with a knurled thumb-screw bearing on its upper surface punch-marks in divisions of tenths of the circumference so as to make possible practically a micrometer adjustment. (2) The pendulum-rods should be made of hard rolled sheet-brass further to reduce the air resistance as is frequently done in clock pendulums. (3) A slotted and hinged arm with some slight spring pressure at the hinge should be attached on that side of the upright support of the chronoscope proximal to the pendulums. This could be released to the horizontal position just above the bobs, thus keeping them in the vertical position when the chronoscope is moved from place to place in the laboratory.

The Sanford chronoscope is still a universally used instrument in our laboratories and shows such promising service in connection with the development of laboratory sections in our elementary classes that improvements of the type outlined in Dr. Dimmick's note are very much worth while. Of course at the same time the modifications must be made as simply as possible in order to keep the instrument within the purchasing power of our smaller academic institutions as well as within the multiplied requirements of our larger universities.

[1] F. L. Dimmick, A further modification of the Sanford chronoscope, this Journal, 41, 1929, 475-476.

[2] C. A. Ruckmick, Some suggestions in laboratory apparatus, *ibid.*, 38, 1927, 647-648.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Colour and Colour Theories. By CHRISTINE LADD-FRANKLIN. Introduction by C. K. Ogden. New York, Harcourt, Brace & Co., 1929. Pp. xv, 287. Nine colored plates.

This volume is made up of the collected papers of Dr. Ladd-Franklin, reprinted from various sources, with minor alterations and additions. Quite frankly (as announced by the editor in the preface) the book pivots upon the 'molecular dissociation' theory of color vison, launched in 1892 at the International Congress of Experimental Psychology in London, as a suggested *rapprochement* between the rival factions of Hering and Helmholtz. This statement, together with the list of 'contents,' will doubtless provoke a momentary pang of disappointment in the reader, hopeful of an ordered résumé of the rapidly expanding mass of visual research, a substitute for the inadequate revision of Parsons' *Introduction.* So utilitarian and impersonal a product, however, lies outside the apparent aim and province of the book before us.

While the Ladd-Franklin theory (owing probably to the uniqueness of its evolutionary features) finds its way annually into an increasing number of texts, few expositions do justice to the subtlety of its original framework and basic chemical postulates. With the initial sketches before us (the requisite emendations indicated usually—not always—by bracketed footnotes and inserts), the groundplan and superstructure of the theory as a whole is clearly manifest, and *ipso facto* the baselessness of many of the strictures passed upon it by Parsons, Troland, and others. One wonders, indeed, as one reads, whether its critics have ever encountered it except at second hand. The reviewer, for one, admits to receiving for the first time, from the perusal of this volume, an adequate conception of the theory, with all its merits and defects.

The book itself, to be sure, provides no single integrated exposition, either of the facts of color vision, of the 'genetic' theory in revised version, or of other theories. Instead, we are confronted by a somewhat disjointed series of 20 or more papers, covering a period of about thirty-five years, from 1892 to 1926; reprinted from *Mind, Science, Nature, Psychological Review,* the *Proceedings of the International Congress of Psychology,* and the like, and including an article on Vision from the *Dictionary of Philosophy and Psychology,* and the Appendix on Color-Sensation from the recent American edition of the *Physiological Optics* of Helmholtz.

The assemblage and collocation of the *membra disjecta* of the theory therefore devolves upon the reader. In spite of the vigor and clarity of diction of the author the task is not an easy one. Nor is it facilitated by certain formal defects in the edition; *e.g.* the lack of typographical uniformity in the serial numbering of papers and plates in the text, and in the Appendix to which one must turn for the date and place of publication, as well as for the description

of the plates. The misprinting of technical terms and proper names is another minor irritant: one picks at random Herzing for Hering, Graeffe for Greeff, 'deulenanope,' 'telrachromatic,' etc. The substitution of phanerogamic for cryptogamic (p. 143) is doubtless a *lapsus calami* on the part of the author; so also the inversion of Donders' postulates in five out of six references to his theory in the text.

Many of the papers reprinted are controversial in tone, protesting the unacknowledge appropriation of portions of the theory, or of one of the author's two original experimental contributions (the demonstration of the normal night-blindness of the fovea, and the disproof of Newton's law of the equivalence of equated lights in all illuminations) by Schenck, König, von Kries, Ebbinghaus and others. The charge of lack of originality, of an unacknowledged debt to Donders is amply clarified—and quashed—by the data here available. The indebtedness to Donders (of whom after all Ladd-Franklin borrowed no more than Helmholtz did of Young, or Hering of Mach) is definitely recognized in at least six papers, including the first report before the Congress. In five of these, to be sure, the basic postulates of the Dutch opthalmologist are curiously reversed, by no means to his credit. In all, however, the use of an evolving color-molecule as a device for harmonizing trichromatic and tetrachromatic findings is frankly traced to his inspiration.

A number of the contributions take the form of a rebuttal of objections, adducing supplemental evidence from recent discoveries in chemistry, histology, and related disciplines. The astuteness and soundness of the original postulates, which have weathered thirty-seven years with minor alterations only, is increasingly apparent. The so-called 'mythical' visual molecule, with its three states or stages of development, is shown, upon the testimony of experts, to be chemically unassailable; a parallel substance, a rosaniline carboxylate has even been discovered. The positing of a single photo-chemical substance, present in different stages of development in rods and cones, and of a single tri-process receptor in the central retina, is substantiated by the later findings of Cajal, of Greeff, and of Hecht, and by certain blue-arc experiments of Ellis, reported in these pages.

When the last word is said, however, the theory leaves the impression on the reader of a logical rather than a psychological *tour de force*. Inspired by successive semesters with G. E. Müller and König, the primary purpose of the author, *i.e.* the effecting of a compromise between the reigning 'components' and 'opponents' schools, has obviously failed of fulfillment. After thirty-seven years of spirited polemic on the part of Dr. Ladd-Franklin, the followers of Hering are still insensible of the cogency of the König color triangle (except as a matter of economy in certain laboratory techniques). The disciples of Helmholtz, also (with minor exceptions), are as 'pre-psychological' as their former leader; as 'psychically blind' to yellow in their accounts of color-blindness; as naïvely confident in the power of mind to achieve the miracle of white from a blend of red-green-blue. In this latter obduracy, indeed, the physicist is confirmed by echoes from the outskirts of psychology itself: the asseverations of McDougall as to the synthetic abilities of the psyche; the easily distorted axiom of *Gestalt*, which asserts the inequivalence of the whole and its parts.

After long and careful scrutiny, the laboratory psychologist is compelled to adjudge the postulates of Ladd-Franklin as little satisfactory as the discarded ones of Helmholtz. The explanations of color-blindness discoverable in these pages are sketchy, involved, and little helpful. The psychological premises themselves on which the older theories are attacked and the superiority of the genetic hypothesis advanced are less compelling than in the nineties. Reliance upon the 'immediate deliverances of consciousness' in the selection of color 'primaries' fell away in the early nineteen-hundreds. Recognition of the simplicity of all color-tones (as such) long since obviated the objection to Hering's 'inexplicable choice' of 'bluish-red' (crimson) and 'bluish-green' (verdigris) as his *Urfarben* (as 'simple' undoubtedly to him—and to most of us—as the lithium or thallium bands of the spectrum to the ordinary eye sophisticated by commercial dyes). Nor is the doctrine of 'psychophysical parallelism' (the obligation to posit similar physical causes for similar psychical effects, *e.g.* for twilight and daylight grays) as sacrosanct as in the decades nearer Fechner. Indeed, the theorist here has been unable to keep faith with her own cardinal principles; abandoning the spectral red for one 'less yellowish,' shifting her green toward verdigris with the passage of the years (oblivious of the fact that the very corner-stone of her system—the generation of yellow by the mixture of orthodox 'unitary' spectral red and green—is thereby subverted). Her insistence upon the 'non-light' cortical or 'background' character of black, in spite of its obviously visual texture and quality, constitutes a virtual abrogation of her second principle, a rigidly interpreted parallelism.

The enduring value of Ladd-Franklin's contribution to visual science, one suspects, lies less in her solution of the Helmholtzian enigma of yellow or of white (the mystery of the 'vanishing color-pairs') through the reversion of the color-molecules, than in her attempt to place both scotopic and photopic vision upon a single unified basis, explicable in evolutionary terms. Her valiant campaign to convert physicist and opthalmologist to a less promiscuous use of stimulus and experiential terms in description also merits gratitude. In the Glossary attached to this volume numerous modifications of current usage, essential if the findings of related sciences are to be mutually available, are suggested: *e.g.* the substitution of achromatic for monochromatic in the designation of total color-blindesss; of *xantho-* or *erythrogenic* radiations for 'yellow' or 'red' wave-lengths when stimuli, not sense-*qualia*, are in question.

A new and striking series of colored plates, distributed through the text (in addition to numerous cuts in black and white), augment one of the chief assets of the theory (according to certain critics), its pedagogic ease of presentation, in schematic form. The plates, to be sure, are of unequal merit, both as regards color-printing, and illustrative design. Plate II, a modified reproduction in color of the König-Helmholtz analysis of the spectrum, reveals apparently too much; suggesting to the reviewer's mind a number of hitherto unnoted inconsistencies in the theory. Plate VIII, showing the color triangle of the tetrachromatic and of the two types of red-green blindness—the 'deuteroxanthic' and the 'protoxanthic'—should prove an eye-opener to certain unreconstructed Helmholtzians. Actually, however, it renders more patent a mystery which would seem to demand a complete recasting of the theory, or

the 'color-molecule.' As it stands the theory explains deuteroxanthia only. As for the articles by other writers, inserted at the end of the book, these appear to add little to the argument, and are below the general scholarly level of the original contributions.

Apart from its permanent scientific worth, the volume is of interest in the side-lights that it throws upon past feuds and schisms still effective in directing the currents of visual research and theory. Particularly illuminating are the sections referring to the era when the recently discovered properties of 'visual purple' gave rise to a ferment of theories afterwards discarded. Whether describing the arrival of an excised human retina at König's laboratory, inventing new illustrative analogies to startle the dull-minded, or badgering her critics, Ladd-Franklin is never dull. The newcomer, especially, in the psychological field will find *Colour and Colour Theories* a welcome addition to his shelves.

Cornell University ELSIE MURRAY

Common Principles in Psychology and Physiology. By JOHN T. MACCURDY. New York, Macmillan Co., 1928. Pp. xvii, 284.

The author states that his object is to suggest a vocabulary in which the principles common to reflex action, instincts and habits, and conscious and unconscious mental activities may be expressed. He actually succeeds in formulating a comprehensive, non-materialistic system of psychobiology. This system was anticipated in his previous study of emotion, but in the present book it embraces in detail the basic principles of both psychology and biology.

MacCurdy postulates the existence of specific immaterial agencies in biological phenomena. These agencies are 'patterns,' and they are due originally to the tendency of all stimuli which effect the organism simultaneously or successively to become united. The result of the initial stimulation is a lowered threshold for one or more of the stimuli. With sufficient repetition the threshold falls to zero, and responses are then made to image 'functions.' The latter are capable of integration into new patterns. All psychological phenomena are to be explained in terms of the integration and activation of patterns.

MacCurdy's 'image' resembles, but it should not be confused with Semon's 'mneme.' Semon, Loeb, and others who believe in some form of protoplasmic 'memory' dispense ultimately with the immaterial factor; but MacCurdy cannot accept this final mechanistic explanation. He regards the immaterial factor as a scientific abstraction, comparable with the abstractions of mathematics and physics. To postulate a factor which functions consistently as an immaterial agent, and then to explain it in physico-chemical terms is illogical. On the other hand, MacCurdy's image is not spiritistic. It is usually subliminal, but at times it is discoverable introspectively. It is not, however, a ghost.

The author's conception of image functions is most clearly explained in his references to behavioristic psychology. To use his illustration, a conditioned response is shown by the man who cowers in the presence of his enemy, for we

cannot assume that this individual has, necessarily, a conscious image of the previous beating. But MacCurdy argues that in the conditioned response the organism acts as if a stimulus experienced in the past were again in operation. For example, he finds that Hunter's 'sensory thought' is in reality his image function, on the grounds that if proprioceptive stimuli are employed after the muscles have been engaged in another response these stimuli must be of an imaginal nature. In general the chain reflex, sponsored by certain behaviorists, is inadequate because it places the organism entirely at the mercy of its environment. But a considerable sympathy for objectivity in psychological thinking is reflected in MacCurdy's theory, in that image functions can be tested objectively. An imaginal process which leads to an immediate response is maladaptive—as when a dog salivates on hearing a whistle, or when a human has hallucinations. But when an image functions indirectly—when, for instance, the dog runs toward the source of food—the action is adaptive. Consciousness is the capacity of the organism to be aware of its own thoughts; objectively, this means the capacity to discriminate between stimuli arising in the present environment and stimuli occurring as reproductions or elaborations of past experience.

Possibly the outstanding feature of MacCurdy's psychology is its affiliation with the *Gestalt* hypothesis. Reaction patterns are images which serve as stimuli for separate elements in overt behavior in the absence of present, material stimuli which ordinarily would evoke the response. Images form, by permitting substitutes, new patterns independently of experience. Attention, perception, learning, and meaning are explained in terms of image function and pattern activation. The motivating factors are by no means ignored; instinct means not only reacting to a stimulus but seeking a stimulus. The latter phenomenon is due to a lowered threshold for a particular pattern. It will be noticed that MacCurdy's patterns, as distinguished from the *Gestalten* of the German schools, are acquired by the integrative process.

The second part of the book is devoted to an interpretation of biological phenomena in terms of the image function theory. Of particular interest to psychologists is MacCurdy's interpretation of central nervous system functions. The basic functions are patterns which can be accounted for only by the assumption of image functions. In brief, they are products of relationships among points of excitation or stimulation; they are, in fact, anatomical 'designs' which are three- and often four-dimensional, for they unfold temporally. At any given time, much of the central nervous system is inactive. Inhibition and repression, as well as other phenomena, find an explanation in the concepts of partial and preliminary activity of dominating patterns.

Because of its scope, this book cannot be characterized adequately in a brief review. Perhaps the chief psychological contribution of its author is his attempt to express the standpoints of functional, behavioristic, purposive, and *Gestalt* psychology, respectively, in terms of his own pattern psychology. A book which purports to accomplish such a task need not be entirely convincing in order to be profoundly stimulating.

University of Texas F. A. C. PERRIN

Gestalt und Sinn. Edited by FELIX KRUEGER and FRIEDRICH SANDER. *Neue Psychologische Studien*, Vierter Band, Erstes Heft. München, C. H. Beck'sche Verlagsbuchhandlung, 1928. Pp. 159.

This number of the *Neue Psychologische Studien*, a periodical edited by Professor Krueger, contains the first two of a series of studies dealing with the general problem of the relationship between configuration and 'sensibility.' The titles of these two studies, with their respective authors, are: (1) Elisabeth Lippert, *Unterschiedsempfindlichkeit bei motorischen Gestaltbildungen des Armes* (pp. 1-83); (2) Carl Schneider, *Untersuchung über die Unterschiedsempfindlichkeit verschieden gegliederter optischen Gestalten* (pp. 86-159).

(1) Miss Lippert reports an interesting series of experiments, showing, in general, that discriminability for a change in the extent of two arm movements made in succession by a subject varies with the degree of unitariness or *gestalt* inherent in the type of movement. She studied three types of movement: (1) a single 'straight-line' movement, (2) continuous movement in a straight line (*i.e.* back and forth in a straight line), and (3) 'round' movement of four sorts (in a circle, in a vertical ellipse, in a standing ellipse and in the form of an unsymmetrical figure). The three varieties of movement are ranked according to unitariness by the author in the order just given, the single straight movement having the least and the curved movement having the greatest 'unity.' The introspections of the subjects confirm this ranking. The quantitative results show, correspondingly, that sensitivity to a change in amount of movement is greatest in the case of the round form (and greatest of all for the circle), intermediate for continuous, straight-line movement, and least for a single straight-line movement. At least, the figures show this after the rather peculiar manner in which the author reduces them to a comparable basis (see pp. 48-60).

(2) Schneider's problem is similar in many respects to that of Miss Lippert. He exposed to his subjects pairs of rectangles in succession for 120 sigma each, and asked them to report whether or not the second rectangle of a pair differed from the first with respect to *gestalt*. He used two general types of rectangles: (1) those 'ohne Binnengliederung', *i.e.* without 'internal organization' or interconnecting parts, and (2) rectangles with such internal organization. The first part of the study is devoted to experiments with 'plain' rectangles. All of the figures were 60 mm. in length, but they varied in height by steps of 5 mm. from 5 mm. to 100 mm. His problem was to find for each subject the limen for sensitivity to a difference in the form of the second of the pair of rectangles as compared with the first. His results show, generally, that discriminability is poorest for the square (60 × 60 mm.) and best for those rectangles approximating the dimensions usually given for the 'golden mean' in aesthetics, namely those with approximately the following dimensions, 38 × 60, and 60 × 98 mm (except for the figures below 20 mm. in height, in the case of which the constant change employed would be greater proportionately than for the higher rectangles; changes in height only appear to have been introduced in the experiment). These 'perfect' rectangles appear to have been the most pleasing to the subjects and changes in their size were most quickly noticed.

In the second part of the experiment rectangles with various kinds of interconnecting lines, *e.g.* diagonals, were used, and the discriminability compared with that for the plain figures. The figures with the 'internal organization' gave a much higher degree of discriminability than was found for the plain rectangles.

The experiments in these two studies have been carefully planned and executed. Although only a few subjects were used (eight, at most, in each study), the main general conclusions, based upon averages, are rarely reversed in the results of any subject. This is a pretty fair test of their accuracy. In general, this type of work, although somewhat limited in its scope, appears more promising than much of the experimentation by *Gestalt* psychologists of the school of Wertheimer.

Vanderbilt University LYLE H. LANIER

Phonophotography in Folk Music: American Negro Songs in New Notation. By MILTON METFESSEL. Introduction by Carl E. Seashore. Chapel Hill, Univ. North Carolina Press, 1928. Pp. x, 181.

In the introduction, Part I, Seashore gives a short historical orientation, quoting largely from his earlier papers presented before the National Academy of Sciences, and explains that the collection here presented is in no sense to be taken as a systematic collection of any type of music of any group of people. It is, on the other hand, only a few random samples intended merely as illustrative of certain possible methods of technique to employ in the recording and study of kinds of ethnic culture which have hitherto evaded scientific preservation. Moreover, the records in these songs are limited largely to only two (tonal and temporal) of the four fundamental aspects of the song, thus omitting the possibilities of amplitude and *timbre* variations. He pleads for the now-possible preservation of the primitive folk traits (now being obliterated) of our Indians, Negroes, Hawaiians, and Filipinos.

The author, a student of Seashore's, describes in Part II his methods of study—the apparatus, methods of recording and analysis, the new "pattern notation" (necessitated by the visual presentation of the detailed analysis), interval and tempo graphs—and defines certain terms to be employed. These descriptions and definitions must be gone over carefully by the reader, with applications to the illustrative selections of artistic singing and of speech, if he is to get the most out of the types of negro songs exhibited in the following part—the workaday religious songs, the more formal spirituals, work songs, 'blues,' and general songs. By means of different colors of ink the individual voices in a quartet, recorded one at a time in different renditions and analyzed in separate voice-curves, are brought together and shown in one staff for the study of harmony effects. Each time, the individual whose voice-curve is to be recorded steps up in his turn to the mouth-piece, while the other three voices, not strong enough to affect the camera, serve nevertheless to give the individual singer in question his bearings. A simpler and more accurate method, it is explained, would be to use wide photographic paper with a separate sound-wave taken for each voice, all simultaneously. The method used necessitated

occasional adjustments to compensate for slight failures to hold to identical pitches through the successive renditions. The results obtained are good and afford interesting and instructive visual presentations of the several parts through their various changes in harmonic relationships.

In all the samples given—some of which are from phonographic records of well known spirituals—various bits of vocal behavior which the ear is too sluggish to reveal stand out conspicuously in perfectly objective records for detailed study at any time—the degree of exactness of pitch maintained on any note, the methods of attack and release, vibrato variations, circumflexes and inverted circumflexes, tempo changes, control of breath, falsetto twists, interpolated tones, etc. These are analyzed and discussed in Part IV. Pictures of the singers, some of them in the various stages of working up the emotions in the 'blues' and in other songs, help make the objective results portray the emotional and the vocal behavior of the singers and suggest the possibilities of the new methods of recording and fixing for study sections of human behavior which are loaded with cultural factors characteristic of different ethnic groups. The pictures also afford excellent opportunity to observe the position of the mouth and lips in the singing of different vowels. It is shown that finished tone-quality is rarely found in negro folk singing. While the negro may enjoy good tones, he in interested more in embellishments and rhythmical devices. His great freedom in singing is not that which is limited by mere aesthetic standards; his limits are physiological only. "At times he will sing as loudly or as softly as he can, his pitch swoops often may be as extended and fast as he can make them, his falsetto-twists as high and quick as possible, or he will sing as long as he can before taking a breath. Then he may go to the other extreme and offset speed with a slow pattern." Monotony is characteristically absent. The negro singer can "jump from one exciting trick to another, and 'nebber tire,' filling his songs with a generous amount of ornamentation."

The closing part of the book, Part V, is devoted to a careful evaluation of phonophotography and to considerations of what is distinctively African and what is American in these songs. It is shown that by the new methods many hitherto troublesome problems of distinguishing between different cultural elements may be successfully attacked in an objective, unbiased manner. This work by Seashore and his students is an excellent contribution to objective psychology, as well as to methods of recording certain bits of vanishing cultures, or behavior traits, which should by all means be preserved.

J. P.

The Psychology of Language. By WALTER B. PILLSBURY and CLARENCE L. MEADER. New York, D. Appleton & Co., 1928. Pp. vii, 306.

It has long been said that the study of the psychology of language has suffered from the fact that those who have had it in charge have been either non-psychologically trained linguists or non-linguistically trained psychologists. In the present volume W. B. Pillsbury, Professor of Psychology in the University of Michigan, and C. L. Meader, Professor of Linguistics in the University of Michigan, have coöperated in presenting in a single volume a treatment of

certain aspects of language. Although the entire book is apparently a coöperative production, the writers have wisely indicated the authorship of each chapter.

The first chapter presents an admirable survey of the general scope of language study. The fields of the science of language are defined. The distinction between general linguistics, comparative philology, and linguistic paleontology, is made. Similarly, systematic grammar and historical grammar are considered, as are phonetics and semantics. Language is throughout considered as a means for the communication of thought, through the instrumentation of nervously directed muscular movement (and glandular discharge) that is capable of being appreciated as significant by another individual. On the basis of this analysis, the nervous mechanism of speech, the speech organs and the senses involved in communication, are considered in a brief and effective manner. In the opinion of the reviewer, however, the treatment of localization of functional centers in the cortex is such as to confuse the reader who has had no previous training in this subject.

The chapter on the mental processes of speech is not in every way convincing. It occasionally seems that Pillsbury forgets that certain words in common use among psychologists will almost certainly be misunderstood by those in other fields—and this book is particularly intended for those students of language who are non-psychologically trained. It appears to the reviewer that the following sentence must almost certainly be misunderstood by the layman: "Speech has its origin *in the mind* of the speaker or writer, and the process of communication is completed only when the word uttered or spoken arouses an *idea* in the listener or reader" (p. 92, italics are the reviewer's). Similarly the *pleasant* results which follow the speaking of certain words are said to impress those words upon the listener.

In later chapters the origin of language, the nature of reading, the process of listening, and the phenomenon of meaning, are considered. The mental antecedents of speech, sound changes in language, and syntax, are also treated.

In general the volume must be characterized as a definite and very valuable contribution to an important field in psychology. To the reviewer it seems that the psychological standpoint is too exclusively that of the classical tradition, but upon this point, of course, opinions will differ. Certain of the views of De Laguna, expressed in her significant recent volume on *Speech*, particularly concerning the relationship between language, behavior, and mental processes, should be considered as an alternative to those presented in this volume. Similarly, the treatment of meaning by Ogden and Richards in their *Meaning of Meaning* should be given consideration in comparison to the views on meaning presented in the volume under review.

A number of misprints have occurred in the volume. The date of publication of Henry Head's major work on *Aphasia* is incorrectly given. Certain men are referred to as "Professor," while others are not so designated. In this small matter consistency in a single volume may reasonably be expected. The present reviewer has a personal dislike for the phrase "the new psychology" in any connotation. It may be of interest to note that in the present volume Configurationism is so characterized.

Brown University LEONARD CARMICHAEL

Elementary Psychology. By ARTHUR I. GATES. Revised edition. New York, Macmillan Co., 1928. Pp. xvii, 612.

Although the revised edition of this book differs considerably from the first edition, much of the subject matter is the same, and the general point of view is unchanged. Even more than its predecessor, the revised edition is characterized by its richness of psychological content. In addition to its more extended accounts of various psychological activities, it contains chapters on personality, individuality, and efficiency. The student who masters it is indeed equipped with a background of general, as well as elementary, psychology.

The Reaction Hypothesis is the basic principle in the author's psychological scheme. It is introduced in the second chapter and is never lost sight of in subsequent discussions of psychological phenomena. For Gates this hypothesis is a pragmatic, and to some extent a methodological, principle: conscious states as well as glandular and muscular activities are reactions. He is thus enabled to adopt structuralistic, functionalistic, and behavioristic standpoints freely as occasions demand, without raising controversial issues.

Such catholicity, however, is attained necessarily at the expense of systematic explanation. Gates is interested primarily in the historical, descriptive, and cultural aspects of psychology. In his chapter on Thinking the author discusses, among many other topics, imagery, 'pure' ideas, motives in reasoning, methods of reasoning, and creative imagination; but he insufficiently explains the mechanism of thinking, and he ignores the one phase of the process which has contributed much to our knowledge of this mechanism; namely, the genetic phase. The author's discussion of Learning goes to the other extreme in that it embraces a number of explanatory principles: both 'Satisfaction' and 'Annoyance,' and joint (conditioned) responses are postulated. It should be said that they are advanced as descriptions of the conditions under which learning takes place. Likewise the account of Intelligence, liberally supplied with factual material, omits any reference to the Spearman-Thorndike issue. In all discussions of higher functions, fundamental explanation is subordinated to description.

The difficulty involved in writing a text which seeks to reconcile traditional material with recent scientific research is reflected in the author's discussion of the nervous system. The theory of synaptic resistance is offered, with little critical reservation, to explain how and why impulses become directed into appropriate pathways. The revised edition is, however, more conservative than the first regarding cerebral functions. The conclusions of Franz and Lashley, omitted from the text of the original edition, are introduced to offset the attempt to correlate sensation, perception, and thinking with circumscribed cortical areas. The reader infers that the cerebrum is not only a connecting device, but the 'organ of thought' as well.

At times the author's interest in classification can be justified more readily on pedagogical than on scientific grounds. For example, while he points out that an 'urge' is not a single and an indivisible entity, his list includes the urges to collect and hoard, to fight for its own sake, and to hunt and destroy. The

reviewer believes that writers of general texts could profitably devote less space to classifications of this sort and more space to discussions of the factors which underlie specific behavior trends.

Perhaps the chief criticism to be made of this really admirable text concerns its author's persistent tendency to distinguish too sharply between 'native' and 'acquired.' It is true that in most instances Gates is unable to classify a given act under either heading; but he seems desirous of so doing. He assumes that the two are logically independent factors. Such an antithetical distinction is questionable in the light of recent critical opinion on the subject. Certainly the psychologist should keep pace with the biologist in the attempt to formulate satisfactorially the issues involved.

University of Texas F. A. C. PERRIN

Educational Psychology. By A. M. JORDAN. New York, Henry Holt & Co., 1928. Pp. xiv, 460.

This book places great stress on data derived from studies of children in the practical schoolroom situation. The author is inclined to underestimate the value to educational psychology of numerous well-controlled investigations in animal, physiological, and experimental psychology. There is sometimes danger in basing conclusions and principles too much on results from situations that are so practical and so poorly controlled that they are purely 'local' and ephemeral. There is, however, no good reason for objection to the use of data derived from practical educational situations, so long as these data are obtained under properly controlled conditions and are checked up with results of experiments dealing with similar processes from different angles and on different individual subjects. Indeed such data are very desirable. The author puts a wholesome emphasis on quantitative and statistical handling of data from the beginning of the book, and, to aid the student in this line, he introduces a special chapter on elementary statistics. This might well have been given as an appendix for convenient reference, and the main facts involved could have been informally brought out in earlier chapters and exercises of the book. Like most educational psychology treatments, this book neglects giving considerable attention to the setting up and testing of hypotheses and to experimental procedures and the control of irrelevant factors. Nothing, for instance, is said of spurious correlations and of the low prediction values of rather high correlations. If we get more carefully obtained statistical data, we shall probably be in a surer way of advancement in psychology.

The general outline of the book is not far different from that of Thorndike's three volumes on the same subject, and the influence of this authority is obvious throughout. Part I is devoted to inherited nature and ability; learning, with application to elementary school subjects as well as to effective methods of study; transfer of training and transfers in school subjects. Part II covers statistical methods and problems relating to individual differences—the extent of differences; sexual, racial, and maturity factors; and influence of family and of environment. The next two parts, which complete the volume, are devoted to intelligence tests and educational tests and to problems of maladjustment and mental hygiene, respectively.

The book is written with a view to use as an elementary text for teaching purposes, and the different chapters contain simple questions and exercises (too general, in the main, probably), as well as references to other elementary works. The author accepts too uncritically the sufficiency of the so-called "laws of learning," originating with Thorndike, and the "normality" of distribution of scores. The reviewer believes that much elementary educational literature has taken up too naïvely certain earlier conceptions developed in the beginning of scientific psychology, largely because of their simplicity for pedagogical purposes, and that it is becoming merely indoctrinary to an amazing extent. There is too much unwillingness to accept probabilities as such and to acknowledge frankly the large elements of ignorance which we still have of many detailed aspects of behavior. Therein lies the danger, for how can attitudes favorable to research and inquiry grow up in such soil? Is education not becoming too much a process of "imparting information" rather than one of stimulating inquiry and critical evaluation of data by objective, scientific methods? The author has chosen to confine the topics of the book mainly to psychological rather than to educational topics, and he has produced a very clear and readable book for elementary work.

J. P.

La conscience morbide. By CHARLES BLONDEL. Second edition. Paris, Felix Alcan, 1928. Pp. 402.

The book is divided into three parts. In the first the author presents observations on seven clinical cases; in the second he generalizes about and systematically interprets his observations and in the last part he presents his theory of the morbid consciousness.

In all the cases that he discusses he finds a striking uniformity of behavior. This is especially clear in those reactions which show what he calls "the motor paradox." Inertia, the refusal of food, etc. are accompanied by scenes of despair and violent excitement. Correlated with this disorder is "the affective paradox."

Patients show great individual difference in their language behavior. The ability to express what they feel varies from a mere mouthing of meaningless phrases to a clear, lucid exposition. However, one is not to accept the latter too literally. By a process of construction and elimination Blondel has correlated the morbid images and the painful cenesthesia of the patients. His conclusion is that the bodily conditions are not the cause but the effect of the images.

As far as the intelligence of the patients is concerned Blondel asserts that there are great variations—from very low to rather high. Thus, absurdity of ideas is not an indication of feeble-mindedness. He postulates a mental state which is out of equilibrium.

In the ability to conceptualize there are also great individual differences, ranging from one patient to whom the past and present mean nothing—living in the presence of some future horror—to another who has no concept of time. Memory capacity varies from that of subjects who have a very well-defined consciousness of the past to that of those for whom the past is only a vague

fantasy. Dominating all these other characteristics is the universal one, morbid anxiety. Blondel asks: "What is this anxiety?" He rejects physiological hypotheses, from both a practical and a systematic standpoint. There are no lesions which show any correlation.

He presents his theory from a socio-genetic point of view. Long analysis has convinced him that the morbid consciousness is different from the normal. The foundation of his theory is the sociological concept of collective ideas (*représentations collectives*). These are ideas which Lévy-Bruhl describes as being common to a given social group, transmitted from generation to generation, residing within the individual and giving rise to sentiments of respect, fear, adoration, etc. Because of this, thinking is socialized behavior. The perceptions of primitive people were enveloped in mystic elements which could not be differentiated, but as man developed, as his language developed, and as the sign language of his emotions developed thinking became exceedingly socialized. Back of all this complex behavior, however, there is the absolutely pure psychological state (*le psychologique pur*), cenesthesia, as opposed to those states of consciousness which have evolved out of it—conceptualizations as shown in words and bodily expressions. In the normal individual this cenesthesia is of a subconscious character, but in the abnormal it is a state of consciousness.

When the morbid consciousness is forced to make its troubles objective in words, this added trouble tends to become one of the elements which strengthens the other elements of the content. This is the condition of the "affective and motor paradoxes." The attempt at analysis of the mass only causes the mass to become more unanalyzable. Blondel, with this as a basis, expands the theory to fit particular cases. In an appendix consisting of two lectures he further presents his theory.

Psychologists eagerly receive any book which advances the scientific status of psycho-pathology. *La Conscience Morbide* is, unfortunately, not such a book. In its theory, which smacks strongly of purposive sociology, scientifically minded psychologists of the abnormal will have little interest. The book, however, has the virtue of being clear, and it may be valuable in its practical applications to work in the clinic.

Clark University CLARENCE H. GRAHAM

The Polish Peasant in Europe and America. By WILLIAM I. THOMAS and FLORIAN ZNANIECKI. New York, Alfred A. Knopf, 1927. Two volumes. Pp. ix, 2250.

The new two-volume edition on the Polish peasant supplants the first five-volume edition published in the years 1918-20. It makes this classic of sociological literature more accessible to the increasing number of social psychologists, sociologists and social workers who find the work indispensable as a model of research and a source-book on methods of approach to the study and interpretation of social phenomena.

Never before and not since has a sociological study been made that in any way can match the scope and value of this treatise. It sets forth a novel and fruitful way not only for the analysis of the immigration problem but of group-

life in general. An exhaustive treatment of the social life and the processes of disorganization and reorganization of the peasant community in Poland serves as a background for the detailed interpretation of the problems of adjustment which the peasant, as an immigrant, faces. A novel method is used by deriving material from first-hand sources; correspondence between members of family groups and autobiographical write-ups. The authors have also offered a new methodological basis for analysis by introducing the attitude-value concept, the four wishes, a new procedure for the development of laws and generalizations and valuable suggestions for the applied sociologist and social worker. From the methodological point of view alone Thomas' and Znaniecki's book is a pioneer work that is destined to play an important part in the development of scientific adequacy in the social sciences. The richness of fertile and poignant suggestions and comments which are to be found in profusion throughout the whole treatise, have not yet, however, been exploited and applied to a great extent, nor is their significance fully realized by the sociologist. It is to be hoped that the new edition will draw a renewed attention to this work.

It is to be regretted that the authors abstained from a revision of the treatise. The original was written over a period of years, during which points of view have somewhat shifted. This made for discrepancies in the text which the new edition should have corrected. Furthermore, the actual text does not show clearly the application of the principles of the "methodological note" which precedes it. The "note" was apparently conceived after the text was completed. But the value of the book could be considerably enhanced by relating with greater precision the conclusions of the text to the methodological propositions.

By way of comment it should be said that the relative contributions of the two authors has for some time been a matter of controversy. In view of the translations of Polish materials that constitute the bulk of the two volumes, and the thoroughgoing knowledge of the Polish background and the 'psychology' of the Polish peasant which the treatise shows, it is evident to the reviewer that Znaniecki has the greater share in the achievement. And so far as the theoretical aspects of the treatise are concerned, Znaniecki's recent publications indicate clearly that his influence in their formulation also was dominating.

University of Illinois THEODORE ABEL

Modern Psychology: Normal and Abnormal. By DANIEL BELL LEARY. Philadelphia and London, J. B. Lippincott Co., 1928. Pp. xiii, 441.

This volume is a text presenting a system of psychology from a strictly behavioristic point of view. The point of view is synthetic throughout and the author continuously emphasizes it as against that of analysis. It would seem to the reviewer that the sub-title of the book is more appropriate than the actual title. The book is more a discussion of personality and the factors influencing personality than a systematic discussion of modern psychology. For example, the whole problem of receptors, and with it the entire problem of

sensory experience and perception, is dismissed in a few pages. Leary is interested almost solely in the more complex integrated processes and reactions.

A reading of the title headings would give one a very inadequate idea of the contents of the volume. Part 1 deals with Fundamental Structures, Drives and Mechanisms, the last two topics coming in for the major part of the discussion. Part 2 is entitled Intelligence: Its Nature and Functioning. In this section Leary discusses the problems of learning, language, "the type of behavior called mental," from both the normal and the abnormal aspects. The title of The Personality and Its Disturbances stands for Part 3. In this section, the treatment is largely from the psychoanalytic point of view, which the author insists has been widely misunderstood by many of its adherents as well as by most of its opponents, because this point of view has seldom been divorced from the emotional reactions of those who discussed it. The last part, under the heading Types and Systems of Personality Adjustment, contains statements of the adjustments of the personality to such matters as beliefs, religion, art and philosophy.

The reviewer believes that Leary has fallen into the all-too-frequent error of the behaviorists of failing to understand just what is meant by systematic experimental introspection. Thus his discussion and criticism of introspection on pages 133 ff. and elsewhere loses much of its point. The sort of verbal report to the question of "What time did you get up this morning?" can only be considered *Kundgabe* and not introspection at all. Furthermore, the statement, "Here, you will find yourself much less glib, unless you are accustomed to just uncritically rolling off a number of words representative in a very dubious sense of this past experience" (p. 134) is just as unfair in its context as it is out of it.

The book contains a number of new treatments of problems such as learning, personality and the social aspects, written, from new points of view. There are a great many footnote references throughout and a bibliography for further reading at the end of each chapter.

University of Pennsylvania SAMUEL W. FERNBERGER

The Psychology of Individual Differences. By R. S. ELLIS. New York, D. Appleton & Co., 1928. Pp. xxiii, 533.

An attempt is made to study individual differences over a wide range of topics. The book is elementary in character, and is more in the line of an extensive summary than a contribution of original ideas. It is written for the beginner or layman rather than the advanced student.

Topics dealt with include mainly the measurements of and differences among mental traits, simple and complex; environment *vs.* heredity; race, sex, and age differences; genius, deficiency, and insanity; brain structure and neurology, glands, and the biology and mechanics of inheritance; and applications of findings on differences to education, law, government, and business.

In general the author applies to nearly every conceivable function the normal distribution curve. Qualitative differences are not admitted except in rare cases, such as physical sex differences, color-blindness, and other sense impairment. All differences are deviations from the normal, in a quantitative

series. To be consistent with the title the author must point out the wide individual differences found, but it seems to the reviewer that he has overemphasized these in not sufficiently recognizing the high correlations which have been established lately between mental traits, particularly the more complex. He does mention the fact of these correlations, but with only minor emphasis.

On the origin of individual differences Ellis's attitude is decidedly nativistic. He says "Environment molds, but it does not create," and it has little effect on the native constitution of the individual. In fact he thinks that the surroundings may even narrow the gap between individuals.

The author in presenting each topic begins (where possible) with its physical aspects, and then takes up the mental. The two, termed "structure" and "function," are considered as extremely closely related.

Social levels and castes are bound to come with the age of a community, as the longer the community has existed the more have the individual differences manifested themselves. An individual usually settles down to a position just about equal to his normal capacity.

In his theories of intelligence he believes more in special ability, apparently somewhat on the group factor basis, than in general ability. Ellis argues against Spearman's *g*, but in doing so he quotes old theories of Spearman which the latter has long since abandoned. "Two individuals with the same mental age and the same I.Q.," he says, "may differ greatly in particular capacities and tendencies."

The suggestion is made, somewhat inconsistently in view of the preceding sentence, to have school classification on the basis of mental age, both in initial entrance and in subsequent promotion. A more homogeneous class would result, which would be better all around, both from the teacher's and from the pupil's standpoint.

A very interesting point is his criticism of the fundamental assumptions of labor unionism, which assumes uniformity of men in ability, skills, and output. This is incompatible with our knowledge of individual differences. A democracy, the author holds, is not very satisfactory; the average intelligence is too low.

Stanford University RICHARD W. HUSBAND

The Phenomenology of Acts of Choice. By HONORIA M. WELLS. London, Cambridge Univ. Press, 1927. Pp. 157.

An attempt was made in this monograph to study the process of choosing by means of introspective analysis under objectively controlled conditions. Reactions and records of changes in electrical resistance, in respiration and impulse were also taken. Particular stress was placed on the part played by affective processes in the voluntary act. One of the points under discussion was the presence of the 'consciousness of action.'

The method involved the presentation of agreeable and disagreeable tastes to which nonsense names were attached in the learning series. After twelve days of learning the tastes the reaction-times were taken for the recognition of the tastes in terms of their nonsense names. In the choice experiments

the observers were asked to select by means of a reaction-key one of a pair of tastes which were presented with the added instruction that they should drink the one so chosen. The monograph goes into detail about the methods of recording bodily expression. There are also extensive quotations from the introspective records with very careful analysis on the part of the author.

One of the outstanding results seems to be the failure of these experiments to arouse any independently developed emotion in the act of choice. True emotions like fear, disgust and anger were not present during the motivation period. They may appear, however, as indicated by the galvanic reflex "after the realization of the decision." Very painstaking introspections are recorded for five different periods of the act of choice. First, the foreperiod; second, the perception of the stimulus; third, the development and description of motives; fourth, the intermediary period; and fifth, the final state or the choice proper.

There are sections on the consciousness of action and the self. The statements of Michotte and Prüm concerning the presence of a consciousness of self and of self-activity are confirmed. Sometimes, however, it appears as untrospectable because of distracting conditions. Whenever the decision is difficult to make this consciousness of self is recognized. In no case was this consciousness of self reduced to kinaesthetic or organic terms. The reviewer can not agree, however, in the charge against Wheeler that an introspective analysis made by his observers revealing these factors was based on a faulty psychology. There is considerable discussion of the various ways in which motivation enters.

The entire paper will bear careful scrutiny and seems on the whole to be very thoroughly done. One naturally wonders again whether differences in the introspective results are not tinged with differences of preliminary instruction in elementary courses. Apparently the observers were very carefully drilled, however, and the results must stand on our records. Apparently we seem to be in the same position at present with regard to the consciousness of self under these conditions as we have been for some time with regard to the thought processes. The self turns up as the thought element did before it in the light of an unanalyzable factor in the conclusions of choice, especially where decisions are hard to make. The reviewer has in his own writing thought wise to distinguish *voluntary* acts and *volitional* acts where the latter emphasize the conscious presence of the self.

University of Iowa CHRISTIAN A. RUCKMICK

Intelligence Tests: Their Significance for School and Society. By W. F. DEARBORN. Boston, Houghton Mifflin Co., 1928. Pp. xv, 326.

The chief purpose of this well-written treatise is to emphasize that the thing measured by intelligence tests does not owe its nature altogether to heredity, but very largely to educational or environmental influences. Instead of analyzing in a thorough-going way the relative importance of nature and nurture in determining existing individual differences, the author marshals a mass of data which he uniformly interprets as pointing toward the importance of environment. The suggestion is favored that there is no fundamental distinction between what has been called innate and what has been called acquired,

but that nature is itself merely a "first habit." One is almost surprised at the end of the book to have the writer speak of a "drawn battle;" yet it must be admitted that the considerations brought forth in this book are no more conclusive concerning the power of environment than are data concerning the significance of heredity, and that there is as much difference between interpretation and proof in the one case as in the other.

A secondary theme is that the Binet intelligence tests involve a very considerable appeal to the capacity to manipulate words, and that they test simply one type of intelligence. Mechanical and social intelligence are mentioned as two other types of intelligence. Besides depending largely upon verbal capacities, a child's intelligence score may be seriously lowered in consequence of some specific disability. Correct treatment of the latter is shown in a number of examples to be followed by marked improvement in the general intelligence scores.

Considerable optimism is expressed concerning the possible effects of improved means and methods in educational procedure. After an excellent analysis of the relationship of intelligence to delinquency, the author concludes that much juvenile delinquency could be avoided by introducing the curricula employed in some reformatory schools into the fourth to sixth grades of city schools, for the benefit particularly of children of I.Q. 85–100.

University of Illinois HERBERT WOODROW

Sex and Repression in Savage Society. By BRONISLAW MALINOWSKI. New York, Harcourt, Brace & Co., 1927. Pp. xii, 285.

In recent years Malinowski has published a series of books on different aspects of primitive society, based upon original researches among the island communities of North-West Melanesia. The study of sex and repression is particularly deserving the attention of the anthropologist and the psychologist alike.

Malinowski's study of the family life of the Trobriand Islanders offers a valuable antidote to the rather uncritical and injudicious application of psychoanalytical theories to the explanation of cultural phenomena, as made by such writers as Freud, Roheim, Jones and others. By comparing the family organization of Melanesian society with that of our civilization Malinowski shows clearly that the so-called "family complex" varies with the constitution of the family and the social stratum. Thus the Oedipus complex "corresponds essentially to a patrilineal Aryan family with the developed *patria potestas*, buttressed by Roman law and Christian morals;" whereas among the Melanesians with their matrilineal family organization and peculiar sex taboos and freedoms an essentially different complex prevails. (Called by him the matrilineal complex.) Malinowski makes a real contribution in pointing out that the typical configurations of sentiments and the problems of repression which the analytical school discovered can not be proclaimed a fundamental attribute of the human psyche. Different conditions and social situations will invariably and, as he shows, by necessity lead to different sentiments and complexes. The consequences of this conclusion are far-reaching since they apply to any attempt at psychological interpretation of human behavior.

To the cautioning notes previously sounded by Rivers, Dewey and Max Weber, there is added by Malinowski additional confirmation of the important methodological proposition that thorough knowledge of the cultural background of a group is necessary before a psychological interpretation can be attempted.

A further contribution is made by Malinowski in the last two parts of the book, in which he shows the inadequacy and fantastic nature of Freud's hypotheses as to the origin of certain cultural phenomena, and complexes in particular. Malinowski instead proposes a much more plausible thesis, built upon the assumption that the beginning of culture implies the repression of instincts by showing that all "complexes" are necessary by-products in the process of the gradual formation of culture.

University of Illinois THEODORE ABEL

Psychoanalysis and Education. By BARBARA LOW. New York, Harcourt, Brace & Co., 1928. Pp. 224.

This book possesses all the vigor and confidence (and perhaps the bias) of a stout defender of the faith. It is an exposition of a normal Freudian view of the dynamics as well as the organization of conscious and unconscious life. Its mission is to present the contributions of Freud to the direct and immediate purposes of education. To this end it explains to the teacher the nature and significance of the Freudian view of unconscious mind, the effect of unconscious wishes on behavior and personality patterns, the interrelations of emotion and intellect, and the mechanism of sublimation. It also presents the case for a large increase in actual use of psychoanalysis as an educational technique. The author points out that a teacher who does not understand himself stands little chance of understanding his pupils, hence true insight into one's self is a first step in a teacher's professional training. If our educational enterprises are to affect the whole child they must not be directed at advancing mere book-learning, nor must they deal with overt behavior and skills in a naïve and superficial way. Unless a child is to drift through school with his very 'self' unknown and hence unaffected except in chance ways, which are often sheer inadvertencies, his teacher must know and deal consciously with the hidden sources of his being. To do this his teacher must know enough about the Freudian view to recognize what is going on within the child and he should refer him promptly to the expert analyst when need for such reference is apparent. Further, certain shifts in emphasis in the whole range of present educational techniques will follow an appreciation of the Freudian interpretation of human life.

The book is well written; it has a message; it makes many a lecture in an average teacher-training psychology course look stale and sour. It deals with a genuine issue and points the way to a whole new set of theorems upon which to base educational practice—if the Freudian interpretation is the way to truth. The reviewer is somewhat impressed with the "if," however.

State University of Iowa F. B. KNIGHT

The Problem of Stuttering. By JOHN M. FLETCHER. New York, Longmans, Green & Co., 1928. Pp. 358.

Professor Fletcher has written from a background of twenty years of experience including clinical and experimental work with the problem of speech defects. His experimental studies were reported in this JOURNAL in 1914. The present volume is non-technical in style and will be found very readable by student and layman. The book may also be recommended to intelligent parents of stuttering children and to the adult stutterer. There is a helpful chapter for the benefit of the latter in the appendix.

The treatment is comprehensive and includes chapters on Classification, Frequency, Causation (with special attention to the problem of handedness), Symptomatology, Prophylaxis, and Environment and Occupational Therapy. The malady of stuttering is interpreted as a social morbidity, chiefly of an emotional nature. Physiological drills of all kinds are held to be contra-indicated. It is also urged that clinical treatments which take the child out of his irritating social situation for short periods, and then return him to the same situation, are out of harmony with the psychological characteristics of stuttering. The author holds that the condition requires revision of the social setting and that the standard and approach in the treatment should be similar to that which prevails for a psycho-neurosis. The author is outspoken in his criticism of commercialism in treatment, and again calls attention to the general neglect from which the stutterers suffer. He believes that there should be division of labor in the study and treatment of the condition, and is a strong protagonist for psychological methods and interpretation.

Yale University A. GESELL

The Psychology of Speech. By SARA M. STINCHFIELD. Boston, Expression Co., 1928. Pp. 318.

Speech Pathology. By SARA M. STINCHFIELD. Boston, Expression Co., 1928. Pp. 252.

The writer of these two volumes has had a wide range of experience with her subject, first, in the correction of speech defects among public school children, and later among college students, as Associate Professor of Psychology at Mt. Holyoke.

The volume of The Psychology of Speech is illustrated with numerous figures showing the neuro-physiological aspects of normal speech. The treatment is of a text-book character and adapted to introductory instruction. Presumably this volume furnishes the theoretical background for the applied volume dealing with speech pathology and with methods in speech correction.

In the latter volume there are many practical exercises, probably safest and most useful in certain kinds of malarticulation. The writer recognizes that such exercises may be a dangerous device if improperly used in the correction of stuttering and other special speech disorders. In both volumes there are chapters dealing with speech measurements.

Yale University A. GESELL

Mental Hygiene. By DANIEL WOLFORD LA RUE. New York, Macmillan Co., 1927. Pp. 443.

The compass of this book is sufficient to introduce the reader to the language of abnormal psychology without previous study in that field. The first of four sections treats the subject of traits of personality and the general nature of mental health and disease. The second section discusses the genetic aspects of personality in its normal and abnormal forms, including the more common psychoses and psychoneuroses. The remainder of the book is given to mental hygiene proper.

The third section gives advice concerning the psychological management of the adult. The more important personal and social adjustments are discussed. The last, and longest, section is devoted to the child and adolescent. One should not go to either of these sections to seek remedies for very specific problems of behavior. Only the general aspects of development are discussed. This detracts from the usefulness of the book for the lay reader who wants to know how to meet particular problems of behavior, but not for the student of mental hygiene for whom the book is intended. The very apt exercises, the pointed questions, the suggestive topics, and the extensive references at the close of each chapter add much to the value of the book for that purpose.

The author has borrowed from a number of points of view and his language is that of behaviorism, psychoanalysis, and purposive psychology. He probably leans most heavily upon the instinctive psychology of McDougall. Freud receives little or no mention, even in those chapters which deal with love and sex. On the other hand, the writer has made frequent references to the works of McDougall and Kempf.

This is a very readable book, with clear and succinct statements, and the author shows himself to be a master of the art of teaching. Its chief fault lies in the lack of observance of strictly scientific terminology. Such unfamilar expressions as 'personality-force' and 'phreno-mental energy' appear from time to time. His fondness for figures of speech leads the author to make such statements as, "The brain battery cannot stand the constant drainage of consciousness but must have time for recharging by sleep." Such expressions arouse concrete imagery for the student, but the encouragement to loose thinking on his part makes them of doubtful propriety.

University of Nebraska J. P. GUILFORD

Psychologie für Lehrer. By OTTO LIPMANN. Second edition, revised and enlarged. Leipzig, Johann Ambrosius Barth, 1928. Pp. vii, 236.

An instructor recently said that her class in elementary educational psychology was not interested in reports of experiments with their accompaniments of tables, diagrams, and statistical analyses. "They want the facts in the shortest, simplest way." If that be the reaction of students in elementary educational psychology in Germany, then Dr. Lipmann has written a textbook that will be popular with students. Psychological findings are turned into axioms. Yet the axioms are presented with such logic that the child, the

subject of the book, is forgotten in the brevity of the rule, which, when the book's obvious purpose is taken into consideration, may be good logic but dubious psychological method.

The chapters are organized after the conventional pattern: sensation, ideational types, reproduction, narration, lies, association and perception are treated before developing the contributions of the Teutonic school to phantasy and the "individual."

The chapter on intelligence and its testing postulates that there is not one intelligence, but rather various intelligences, yet still claims the three mental types: the creative, the concrete, and the composite imagery types. One supposes that Bobertag's revision of the Binet-Simon Scale is the only test that the central European countries know about. At least, it is the only one discussed, and its advised use is limited to selecting pupils for free places in schools. This omission may be an expression of disinterest in, or perhaps, disbelief in, the technique of mental and educational measurements.

As a textbook, this potpourri of axiomatic renderings of G. Stanley Hall's unproved if brilliant guesses, of early Wundtian psychology, and of the psychology of the "individual," is hardly adequate to any needs such as are felt in classes in American teachers colleges and in departments of education in universities.

Institute of Educational Research
Columbia University IRVING LORGE

Psychologie der frühen Kindheit bis zum sechsten Lebensjahre. By WILLIAM STERN. Fifth edition. Leipzig, Quelle & Meyer, 1928. Pp. xv, 539.

In attempting to present in this edition the various contributions of recent investigations in child psychology, the author has necessarily made the book diverge somewhat from the original form of the work. One of the chief contributions of the present edition is the greater elaboration and clarification of the author's view, that the child must be considered as an evolving personality, rather than as a mere composite of separate functions and traits. From this organic-unity or personalistic point of view, which he has stressed of late years, the author considers the recent contributions by investigators of thinking and intelligence and of the development of the child's esthetic life and enlarging ideational system. He also evaluates critically from this point of view the contributions of *Gestalt* psychology, of differential psychology, and of psychoanalysis. Several chapters have been largely rewritten in the light of the rapidly growing modern literature on experimental and observational studies of children's behavior. It is, of course, hardly to be expected that the numerous recent contributions by American investigators in this and in related lines will be largely and thoroughly incorporated in such a revised volume. Several important studies, however, come into consideration here and there. The book gives a rather inclusive summary of the behavior-traits and modifications of the growing child as revealed from a broad genetic point of view. The bibliography of this edition covers over ten pages, mostly of references to European literature.

J. P.

Clinical and Abnormal Psychology. By J. E. WALLACE WALLIN. New York, Houghton Mifflin Co., 1927. Pp. xiv, 649.

This book should put a wholesomely dampening influence upon the unhealthy curiosity that a great many students bring to the study of the abnormal. Clinical and abnormal psychology is treated as a dignified branch of science which deserves the attention of mature students of education and psychology. The book may serve equally well as a text-book or as a reference book. To one who has been exposed too early to the belittling influences of psychiatric promoters toward clinical and abnormal psychologists, this book should serve as a strong stimulant toward helping him see the value and the application of his chosen field. Although the book appears relatively small it is in reality large. The method of presentation of the material has aided appreciably in increasing the amount of ground covered. Wallin has produced a fine mixture of clinical and abnormal psychology which indicates clearly how we should think about the practice of psychometry. The clinical psychologist should have a background in education and sociology as well as training in all branches of psychology and psychiatry. The usual criticism may be offered, namely, that certain aspects of the field are inadequately handled. Clinical psychology has grown to include a study of all kinds of unusual behavior, such as occur in cases of special disability, emotional maladjustment, speech defect, physical handicap, etc., so that any one particular specialist might feel that his field is slighted in this book. But when one part is considered in connection with every other part of the book it will be apparent that there is no great error in the balance and proportion of parts.

A bibliography, limited to the more important recent text-books, treatises, and monographs appears at the end of the book.

University of Iowa LEE EDWARD TRAVIS

Practical Psychology: For Students of Education. By CHARLES FOX. New York, Harcourt, Brace & Co., 1928. Pp. xvii, 180.

The title of this book is misleading. One would never suspect from the name alone that it is intended to be a laboratory manual in educational psychology. The only excuse for its title is in the fact that the author aimed to make the subject-matter "as concrete and as little artificial as possible, consistent with scientific treatment."

The experiments, 28 in number, are of a semi-popular nature. They require little or no apparatus outside the book itself, and they are so intensely interesting that the reviewer could not refrain from doing some of them during the first reading of the volume. Indeed, they might easily furnish the basis for an evening's social entertainment. Yet, the students will not fail to get the point of each experiment in the laboratory, and the methods of treating results leave nothing to be desired, even by the most critical psychologist.

The subjects covered, and the number of experiments devoted to each one, are as follows: sensory acuity, 2; perception, 2; habit-formation, 5; imagery, 5; association and memory, 4; suggestion, 3; mental tests, 4; practice and fatigue effects, 3.

Most of the experiments are new, and yet they have been used long enough in the Cambridge laboratory to establish their value and to set up norms wherever norms are desirable. The author insists upon rigid quantitative treatment of results everywhere possible. To assist the student in this, Part II of the manual is devoted to statistical methods, including measures of relationship, averages and deviations. The very brief and yet advanced treatment of these subjects stands in sharp contrast to the elementary nature of the experiments themselves.

The usefulness of the manual would undoubtedly be enhanced by a correlated use of the same author's *Educational Psychology*. While it may not be adaptable *in toto* to American soil, certainly it will be very suggestive to anyone looking for new ideas in laboratory procedure in educational psychology.

University of Nebraska J. P. GUILFORD

Handbuch der Individualpsychologie. Edited by ERWIN WEXBERG. München, J. F. Bergmann, 1926. Vol. I, pp. ix, 664; Vol. II, pp. 200.

In two volumes thirty-one different authors contribute here to a handbook of individual psychology. The basic conceptions of Alfred Adler are used here not only in the fields of education, child psychology, and psychopathology, but also in considering such fields as ethics, religion, and philosophy. The reader will be hardly convinced that a reorientation of these various fields from the point of view of individual psychology has been achieved; the book rather shows what happens if theories formulated in connection with the development of therapeutically effective measures are suddenly introduced into such heterogeneous fields as politics, sexology, criminology, and philosophy, etc. The reader learns how the authors impressed by the importance of the *Leitlinie*, the *Gemeinschaftsgefühl*, etc., "feel" about the various kinds of scientific and non-scientific activity. The book does not show what is *gegeben* scientifically from a new point of view, the point of view of individual psychology, but demonstrates what is *aufgegeben* educationally, therapeutically, socially, and politically. Whoever has to handle human material will find a wealth of observations and suggestions in this book. The "bibliography of individual psychology" containing 337 references will be much appreciated.

Institute for Juvenile Research,
Chicago, Illinois HEINRICH KLÜVER

The Psychology of Learning: An Advanced Text in Educational Psychology. By. W. H. PYLE. Revised and enlarged. Baltimore, Warwick & York, 1928, Pp. ix, 441.

The revisions are slight. The original point of view and general outline remain. Two chapters are added, one on the Psychology of Special Subjects, consisting of five pages of statements which are so general as to be valueless; the other, on A Theory of Learning, is only a reprint of the article by the author published in *Psychological Review*, 1924. Neither chapter contributes any data whatever, or any suggestions as to scientific methods. The main additions in this revision are an enumeration of certain "general principles" and illustrative cases of their application, at the conclusion of each chapter. Many

of the alleged principles and examples are put in the form of dicta: "The teacher should," etc., and "The child should," so and so. These are merely arbitrary statements. If much time is to be spent on learning these "principles," which is a danger when the class is in poor hands, the book will surely do little to advance scientific work in psychology. It may do much harm by encouraging dogmatic attitudes. There is no obvious reason why data for educational students, and scientific methods, should not be as well established as those for other students. Bibliography and references at the end of chapters are brought up to date, but are evidently not intended to be complete.

J. P.

Children's Behavior and Teachers' Attitudes. By E. K. WICKMAN. New York, Commonwealth Fund, Division of Publications, 1928. Pp. 247.

This is an exceptionally able and well organized inquiry into the nature and frequency of misbehaviors in ordinary school life together with readable presentations of the basic data and prudent interpretation of the data. That teachers should revise their appraisals of the relative seriousness of several 'offenses' or that our clinical experts are seriously incompetent to judge the true significance of many types of misbehavior is evident. The discussions on the analysis of the misbehaviors and teachers' attitudes toward them seem to the reviewer both forceful and correct. This document furnishes a much needed array of data for use in counseling teachers on the problems of misbehavior and its causes. We may yet persuade teacher-training institutions to concern themselves with the dynamics of human nature more and the dry bones of formalisms less, and this book will contribute valiant service to that end. The statistical treatment is on the whole sophisticated and a refreshing relief from the naïve incompetence of much work in this field. In the opinion of the reviewer the Commonwealth Fund got more for its money in this study than it often does.

State University of Iowa F. B. KNIGHT

Laura Bridgman: The story of an opened door. By LAURA E. RICHARDS. New York, D. Appleton & Co., 1928. Pp. xx, 155.

Now that the original accounts are out of print, psychologists and educators will welcome this well-written story of the development of one who was blind, deaf, dumb and virtually wanting in taste or smell. Mrs. Richards does not attempt an interpretation of the incidents she recounts, but her material suggestes the functioning of highly dynamic forces, often intensified by Laura's lack of normal human intercourse. One can plainly see energetic self-assertion with typical revolt at restraint, and compensation through efficient execution of activities within her powers; high satisfaction in established routine and severe attacks of homesickness when her environment was changed; craving for affection and some incipient romances; fairly clear stages in the evolution of a conscience and an intense interest in religious questions whose natural development might have contributed to our knowledge had not an over-zealous teacher imbued her with Calvinism.

Mt. Holyoke College S. P. HAYES

The Technique of Controversy: Principles of Dynamic Logic. By BORIS B. BOGOSLOVSKY. New York, Harcourt, Brace & Co., 1928. Pp. viii, 266.

This purports to be a "grammar of thought." The author derives from conspicuously successful examples of modern reasoning—Maxwell's electromagnetic theory of light, the concepts of species in biology and elements in chemistry, Dewey's interest and effort—a dynamic logic in contrast to the classical treatment of the subject which he labels static logic. The ancient sophism and the modern paradox are illogical only in that they contradict this static logic. Actually they conform to a world of ceaseless but interrelated change. Bogoslovsky revises the rules of reasoning, in general, to conform to such a world.

The chapter on principles of dynamic logic gives a good discussion of the psychology of reasoning. Reasoning as a process is generalized into an ingenious formula utilizing mathematical notation. The application of the principles to present day problems is characterized by apt illustrations and keen psychological insight. No small amount of controversy, particularly in the various fields of social science is traced to the attempt to treat relativistic and changing phenomena according to static canons of logic. The principles of dynamic logic provide not only an explanation of the origin of many controversies but a means of resolving some of them. Probably a new "grammar of thought" will no more insure accurate reasoning than other grammars insure correct use of language, but so far as a new grammar is better founded than the old ones it becomes a more useful criterion against which to evaluate differences of opinion.

University of Oregon HOWARD R. TAYLOR

Notes on The Originality of Thought: The Concept of Internal Necessity: Poetic Thought and Constructive Thought. By LEONE VIVANTE. Translated by Professor Brodrick-Bullock. London, John Lane, 1927. Pp. 221.

This work, which was reviewed by the undersigned when it appeared, in its original Italian form, is now made available in English. Although the reviewer is not competent to judge the adequacy of translation in every small detail, he is impressed by the facility of expression and the clarity of the presentation in spite of the abstruseness of the subject. As a psychological inquiry the book has relatively little value from an empirical point of view. The theoretical analysis of poetic thought follows the Bergsonian interpretation but from a new angle.

University of Iowa CHRISTIAN A. RUCKMICK

Social Work and the Training of Social Workers. By SYDNOR H. WALKER. Chapel Hill, N. C., Univ. of North Carolina Press, 1928. Pp. xiii, 241.

The present status of the social worker is well delineated in this study which should prove of practical value to those who contemplate entering the field of social work. The book is illuminating to those who seek general information as to its problems and philosophy. A bibliography of thirty-five pages is appended.

University of Illinois HERBERT WOODROW

Sexualtyp und Kultur. By ERNEST KLIMOWSKY. Berlin, A. Marcus and E. Weber, 1928. Pp. 63. (Abhandlungen aus dem Gebiete der Sexualforschung, Band V, Heft 3.)

Within these sixty-three pages the author has attempted to present the sexually determined characteristics of men and women as they are revealed by music, literature, and the other arts through all the years from about the eleventh century to the present. In its brevity there lies the virtue of leading the reader to envisage easily the changing traits of the male and the female down through the centuries. At one time masculine traits predominated, at another feminine and again the two are equally balanced. But a larger study of the same subject might have been more convincing. There is no systematic survey to determine just what is masculine and what is feminine. That is taken rather for granted and traditional distinctions are used. The many references and quotations indicate a commendable erudition, but the psychologically trained reader will marvel at the omission of nearly all of the well known literature on the psychology of sex.

University of Oregon EDMUND S. CONKLIN

Directory of Psychiatric Clinics for Children in the United States. ANON. Second edition. Foreword by GRAHAM R. TAYLOR. New York, The Commonwealth Fund, Division of Publications, 1928. Pp. 181.

This new edition, which is the work of the Division on Community Clinics of the National Committee for Mental Hygiene, will be welcome by psychologists, psychiatrists, educators, etc., because the first edition (1925) has been out of print for some time. The first edition was published by the Joint Committee on Methods of Preventing Delinquency, in coöperation with the National Committee for Mental Hygiene; but with the termination of the demonstration period of the Commonwealth program, the first named of these two committees was discontinued and its activities were transferred to the newly created Division of Publications of the Commonwealth Fund. The Directory contains information gathered during the three months ending June 1, 1928, as well as certain additional information which became known even later to those persons handling the revisions, and it is therefore fairly representative of actual conditions at present and will be serviceable for several purposes.

J. P.

The Free Association Test (Kent-Rosanoff). By AARON J. ROSANOFF. New York, John Wiley & Sons, Inc., 1927. Pp. 546-620.

This is a reprint, separately bound, of Chapter VII of the sixth edition of the *Manual of Psychiatry,* edited by the author. The sixth edition has already been reviewed in the *Journal* by Edmund S. Conklin (cf. p. 130 above). The present chapter contains the same material as Appendix VI of the fifth edition (1920), and also four new pages of Juvenile Reactions. These new lists include all the reactions given in Woodrow and Lowell's *Children's Association Frequency Tables* (1916) that have a frequency value of over 0.1%. Doubtful reactions have not been included here.

J. P.

LE RÊVE. By MARTIN GOMES. Rio de Janeiro, Rodrigues and Co., 1928. Pp. 179.

Here is a book on dreams which has little to say about the unconscious and from which practically all of the usual psychoanalytic machinery has been omitted, although the author is obviously not ignorant of the psychoanalytic literature. But the reader is forced to worry through more than half of the book before he is permitted to look at the nature of the dream; and even then he does not find much to reward his perseverance The dream is presented as a peculiar pattern of images and sensations, which undergoes alterations according to certain laws of association and elimination and combination. The unique and striking feature of the book lies in the author's attempt to develop a system of symbols for the representation of conscious patterns. These are at first intriguing, and eventually annoying. Many of the pages are so littered with what appear to be elaborate formulae as to remind one of a current text in statistical methods. But there is no attempt to make real equations or algebraic formulae out of the symbols. They are rather schemes for a graphic symbolic representation of the structural elements involved in a state of consciousness, and to some extent of the intensity values of these elements. Although many dreams are described and the style implies at times that an inductive study is under way, the book could not justly be classed as a systematic inductive study of dream experience.

University of Oregon — EDMUND S. CONKLIN

The Inner World of Childhood. By FRANCES G. WICKES. Introduction by Carl G. Jung. New York, D. Appleton and Co., 1927. Pp. 379.

The 'inner world of the child' was laid bare to a woman who was not only a sympathetic mother and teacher but also a discerning observer. Such aspects of the life of the child as depend upon the child's reactions to his early relationships, and also such topics as repression, psychological types, imaginary companions, fears, sex life, phantasy and dreams are discussed.

The book will be of great value to those who have to do with child guidance or who want a guide to aid in interpreting the 'queer' things that children do, but it will be of use to psychologists only for its illustrative material which is rich with typical cases and interpreted with rare understanding.

University of Nebraska — J. P. GUILFORD

Influencing Men in Business. By WALTER D. SCOTT. Revised and enlarged by Delton T. Howard. New York, The Ronald Press, 1928. Pp. 172.

This is the third edition of Scott's popular work which was first published in 1911. The chief change is the expansion of one chapter into three, as follows: Deliberation and Suggestion Contrasted; Types of Decision; The Several Types of Suggestion. Several new cuts (illustrative advertisements) are added. A new format is adopted, and new type face.

Columbia University — HARRY D. KITSON

Familienforschung Vererbungs- und Rassenlehre. By ROBERT SOMMER. Third edition. Leipzig, Johann Ambrosius Barth, 1927. Pp. vii, 519.

In an attempt to show the intrinsic relations that exist between race hygiene and genealogical research, to lay a deeper and broader foundation for that genealogical inquiry, Sommer has written an excellent introduction to the methodology of descent indicative of its application not only to individual psychology but also to criminology, psychopathology, and anthropology.

Sommer illustrates his techniques in his own lineage and in that of his wife whose ancestral line goes back to that of the Soldani who are mentioned in Dante's *Divine Comedy.*

In the second part of his volume, Sommer elaborates his individual genealogical method to apply it to racial extractions.

The book, on the whole, surveys the biology of inheritance, the technique of lineage, making an important contribution to the procedure of lineage-tracing.

Institute of Educational Research
Columbia University IRVING D. LORGE

Serving the Child in Fargo. ANON. Part three of the Final Report of the Fargo Demonstration. New York, The Commonwealth Fund Division of Publications, 1928. Pp. 127.

This book gives an account of the five-year program concerning child health conducted in Fargo under the auspices of the Commonwealth Fund. The general program is described, and numerous graphs and tables showing the scope of the work, the incidence of various defects preceding and during the health demonstration, infant mortality rates, etc. are presented. There are a number of interesting photographs illustrating the type of work done. An itemized statement of the cost of the program is appended.

Institute of Child Welfare FLORENCE L. GOODENOUGH
University of Minnesota

Newman on the Psychology of Faith. By SYLVESTER P. JUERGENS. New York, Macmillan Co., 1928. Pp. xiii, 288.

A scholarly consideration of those writings of Cardinal Newman which bear on the problem of how the individual comes to give assent to any proposition, and particularly how the individual gives assent to what is termed the Christian revelation. Bibliography, elaborate and informative notes, and an extensive index are appended to the treatise proper.

Brown University LEONARD CARMICHAEL

Psychodynamique generale. By ALFRED LARTIGUE. Paris, Félix Alcan, 1926. Pp. iv, 138.

Of no interest to the psychologist, except perhaps as an instance of baseless speculation.

Harvard University J. G. BEEBE-CENTER

NOTES AND DISCUSSIONS

'Observer' and 'Subject'

A recent proposal, made by an exponent of "consistent objectivism," to remove from the psychological laboratory the term *observer* and to retain only *subject* has suggested the following comment upon the history and the significance of these two words. Since the terms have long been carefully distinguished in many laboratories and by certain of our psychological periodicals, this comment will only formulate and discuss that which is already widely recognized in current practice.

Where the chief results of experiment have appeared in the form of verbal report, made under instruction, it is natural that the historical word *Versuchsperson* should have been commonly rendered 'observer.' There the psychological matter-of-fact is observed under a tacit agreement between *the experimenter*, who carries the general responsibility for the arrangements and the procedure and gives signals and instructions, and *the observer*, who works with whatever knowledge and sophistication the character and the aim of the problem dictate. The observer adjusts himself to the experimental situation, observes responsibly and with intent, and reports in prescribed terms and under prescribed rubrics. Although the observer's reports have frequently been made in terms of 'consciousness' and in the form of 'introspection,' these terms have absolutely nothing to do with the main functions of the psychological observer, who has precisely the same experimental significance and validity whatever the terms and the categories of report. Herein lies the persistent error of the 'objectivist' who naïvely insists upon identifying all reports of observers with 'subjectivism' and 'introspectionism.' Can it be possible that he has never learned at first-hand the actual experimental procedure of reporting in the psychological laboratory? It is difficult otherwise to understand his confusion. Some one must report in the psychological experiment. That is as obvious as it is in the physical, the chemical, and the physiological experiment. The point of the objectivist seems to be that he prefers to do all the reporting and recording himself and not to trust another. What he demands in his laboratory is the *Versuchstier*; some creature which he can 'put through' or 'run,' as he colloquially phrases it. Since that sort of behaviorism came out of the animal laboratory and from the Cattellian anthropometric test, it is natural that the paradigm of procedure should be the rat's maze and the freshman waiting line. The creature so 'run' pantomimes his part, cautioned not to say anything lest he introspect or prattle of conscious vacuities. At most he is permitted to verbalize with his laryngeal muscles or mutely to gesticulate in order to 'express' what is swelling within him. This creature the objectivist prefers to call *the subject*, overlooking the mild inconsistency between having subjects and rejecting with phobiac scorn everything 'subjective.'

In those experiments where *the observer* observes and reports, the setting is different. There a part at least of the results come out in the reports. The observer need know nothing of consciousness or of the various historical

forms of introspection; but he must apprehend the experimental situation, he must comprehend the rules of procedure, he must take the experimental attitude, he must intelligently understand formal instructions set him by the experimenter, he must recognize the influence upon the experimental outcome of his surroundings (occasional instruction) and must detect the various and insidious forms of self-instruction which modify his reports, influence the amount of his accomplishment, and determine the character of his behavior. Finally, the observer must possess the linguistic resources necessary to accurate and appropriate recital. In a word, the psychological observer is always specially and technically trained; but his training is in principle the training of the reliable observer in any of the descriptive sciences. Some laboratories do doubtless speak still in the phraseology of mind, consciousness and introspection; though their number is obviously decreasing. If they make good psychological use of these concepts, there would seem to be no sufficient reason why the ardent objectivist should question their methodology. The point in the present context is that the employment and the training of the psychological observer have nothing in the world to do with these or with any other 'subjective' terms.

Now it is obvious, both from the logic of experimentation and from the actual output of our laboratories, that there are certain problems which are properly approached by the 'running of subjects,' human and animal. These are generally problems which may be phrased in the stimulus-response relation. They have been in all our laboratories for a half-century. All responsible experimentalists are able to deal with them. They require no special doctrine and no allegiance to a cult of objectivism. Their point of view was clearly stated by Herbert Spencer in 1855. The experimenter's interest is fixed upon environmental change, and the organism is regarded as a more or less constant third term set between the stimulating agent and the resulting performance or behavior. Here the organism may properly be regarded as 'subject.' But it is again just as obvious that the canons of experimental control and record make such an assumption inadequate and unprofitable in many problems of a different character. This is true wherever the organism enters the scene as *agent*, whether experiencing agent or agent of some other sort. There it is important that the history, the momentary disposition and attitude, the forward-running tendency and intent, and the general resources of the organism should be experimentally accounted for; and there it is usually most profitable to regard the organism as an observer and to make its functions of observation and report complementary to those of the experimenter. In many cases no other means have been provided for discovering many important factors which are involved in the experimental situation.

It is not to be expected that psychologists of all schools and of all parties will succeed in using in a competent way both 'subjects' and 'observers;' but it should be possible for all experimentalists at least to understand the distinction. Possibly we may hope that when the distinction is more clearly apprehended by the 'objectivist,' he will be willing to modify the old and badly conceived opposition between himself and an imaginary body of 'subjectivists.'

MADISON BENTLEY

A Delayed Visual After-Effect

In a note published in 1921 on certain unusual visual phenomena[1] I described a succession of delayed 'positive after-images' which occurred some 15 minutes after viewing under the microscope some slides showing sections of the cerebellum. The phenomena were vivid and shifted constantly, the observed movements corresponding apparently to the actual movements of the slides in the original experience. There was no way, however, of determining whether the order of presentation actually followed the order of the original. A recent experience demonstrates this to be the case.

Immediately after luncheon I lay down for a nap. At this moment our dog, a cocker spaniel, came up and slowly raised his head toward mine. I responded by lowering my head slowly toward him. Soon afterwards I closed my eyes and slept some 10 or 15 minutes. On awaking, before I opened my eyes, there appeared a very distinct image of the dog's head, in white with brown spots and dark eyes —*i.e.* in proper shape, color, and size. The head slowly grew larger, just as in the original experience, and then disappeared. The whole experience lasted a few seconds only.

This experience seems to demonstrate conclusively that a vivid positive after-sensation (or image) may appear after an interval of several minutes occupied by sleep, and that it develops (changes) in the same order as the original sensory figure. On account of the distinctness and sharp outlines of the picture I am of the opinion that the phenomenon is of peripheral origin—an after-sensation, not an after-image. But I can see no way of proving this. I have found few allusions to this type of phenomenon in the literature, and none so crucial as the above. It might be worth while if others were on the look-out for similar experiences.

Princeton University Howard C. Warren

Ninth International Congress of Psychology

The Ninth International Congress of Psychology met at Yale University in New Haven from September 1 to 7, 1929, under the presidency of J. McKeen Cattell. It was the largest meeting of psychology that has ever been held. There were 826 members in attendance, besides 211 wives of members and other guests. There were 104 foreign members, all but 17 of them from Europe. The rest were Americans. The total membership, including those persons who joined the Congress and yet did not attend, is 1220. This figure may be compared with the corresponding figures of 239 members at the Oxford Congress in 1923 and 241 members at the Groningen Congress in 1926. Although all members were delightfully housed together at the Harkness dormitories, there can be no doubt that the Congress lacked the intimate contact of preceding Congresses. The many gained at the expense of the few.

The program reflected the size of the Congress. It included about five hundred papers and addresses.

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[1] H. C. Warren, Some unusual visual after-effects, *Psychol. Rev.*, 28, 1921, 453-463.

The Congress was formally opened by an Address of James R. Angell, President of Yale and Vice-President of the Congress. J. McK. Cattell followed him with a presidential address reviewing the development of American psychology. E. Claparède, as Permanent Secretary of the Congresses and the only continuing officer, followed with an account of the history of the Congresses.

In the evenings there were formal addresses by I. Pavlov, W. Köhler, A. Michotte, H. Piéron, W. Stern, M. Ponzo, C. Spearman, and E. L. Thorndike. On Wednesday evening, K. S. Lashley delivered the annual address of the President of the American Psychological Association on "Basic Neural Mechanisms in Behavior."

In the mornings there were thirteen sessions for the reading of the more formal twenty-minute papers. These sessions were classified under the subjects Theory and History of Psychology, Comparative Psychology, Educational Psychology, Methodology and Technique, Social Psychology, Child Development, Physiological Psychology, Psychology of Personality, Abnormal and Clinical Psychology, Experimental Psychology, and Psychology of Industry and Personnel.

The afternoon sessions were devoted to informal symposia or round-tables, where short eight-minute papers were read and discussion was emphasized. There were thirty-one of these symposia on the following topics: Animal Behavior, Applications of Psychology to Methods of Teaching, Effects of Drugs, Industrial Psychology, Maladjustments, Psychogalvanic Reflex, Theoretical Psychology, Character and Personality, General Intelligence, Legal Psychology, Meaning and Symbolism, Psychophysics, Abnormal Psychology, Child Development, Eye-Movements, Laboratory and Teaching Devices, Memory and Learning, Psychology of Music, Race Differences, Nature of *g*, Personnel Psychology, Physiological Psychology, Social Psychology, Statistical Method, Sleep, Aesthetics, Mental Tests, Motivation, Reflexes, Religious Psychology, Sensation and Perception. Many of these symposia were so large that they had to be divided into simultaneous or successive sessions.

In the face of this gigantic program, far exceeding the physical possibility of attendance by any one person, it is difficult to formulate any statement of the general significance of the Congress. Nevertheless, several very gross generalizations appear.

(1) The mere size of the membership and of the program show the present growth and health of psychology.

(2) The Congress gave some indication of the present status of America in the psychology of the world. The Congress had over a thousand American members, in spite of the fact that the secretaries rigidly excluded all persons who lacked the professional status which would have rendered them eligible for Associateship in the American Psychological Association. In no other country, nor presumably in all Europe together, could this condition have been duplicated. America leads psychology in numbers. Whether it also leads in the cumulated quality of its psychological research is not for the present writer to indicate.

(3) The large program will make the Proceedings a cross-section and a census of the psychology of the present day, certainly for America, and somewhat inaccurately for the world at large. The topics for the sessions were only in a small measure determined by the Program Committee; largely they were the result of a classification of the papers received and thus a census of the range of interests of the membership. What was psychology in 1929? The answer to this question will be the exhibit of the record of this program.

(4) One other generalization the writer ventures. The program seemed to him to show the increasing dominance of physiological, behavioral psychology in America. The day of behaviorism in America is over. It has accomplished its purpose, and American psychology now takes of physiological or behavioral data whatever its problems need. The same tendency seems to be not nearly so marked abroad, so that American psychology is now apt to have a flavor different from most of the European psychology.

The next Congress will be held in Copenhagen at the end of August, 1932. The Ninth Congress expressed its hope that Professor Höffding could be president of the Tenth.

E. G. B.

Illusory Movement from a Rotating Barber Pole

If one observes a rotating barber pole, preferably one of the type which is enclosed in a glass tube, three possible phenomena may occur. (1) The observer may see the movement as it objectively occurs, that is a torsion which is conditioned by the turning of the pole on its vertical axis. The diagonal stripes are taken for what they actually are, namely, spiral stripes on a revolving cylinder.

(2) The observer may see movement in the *vertical* direction only—an illusion which is conditioned by the spiral stripes as they appear to rise upon the turning pole. In this case, horizontal movement is missing. The pole seems to be sliding smoothly upward, and there is for the writer an imaginal extension of the pole above and below the glass case. The total perception is of a non-rotating pole of indeterminate length which is being shoved upward through the glass case. The extension of the pole is one of meaning rather than of visual quality. It is as if the portions of the pole above and below the glass case went out of view behind something, but there is the impression of movement behind that something.

(3) The observer may see a combination of the horizontal and vertical movements, with one of the two components prevailing to a greater or lesser degree. If the vertical component dominates, the stripes themselves seem to move upward on the surface of the pole, but the pole itself does not move upward as in (2). If the horizontal movement is dominant, the stripes seem to move diagonally upward in a direction at right angles to the stripes themselves.

The three phenomena may fluctuate, one giving way to another, as in the case of all ambiguous illusions. The fluctuations are also subject to the control of mental set, just as the fluctuations of the ordinary ambiguous figures, like the Necker-Wheatstone cube or the Schroeder staircase.

The writer has no objective proof of the rôle of eye-movements in these phenomena, but he believes that they have a great deal to do with the prevailing pattern of movement, or are at least a sufficient condition for them. Experimental registration of eye-movements is needed to decide the matter.

University of Nebraska J. P. GUILFORD

THE RATE OF THE HICCOUGH REFLEX

Last spring I suffered an attack of hiccoughs which persisted for over forty hours. The attack began at 8:30 A.M. on April 21, and ended during sleep sometime after 1:00 A.M. on April 23. During the first day I directed my efforts toward obtaining a cure, but when it became evident that none of the numerous 'cures' that I tried would be immediately effective I decided—particularly as I could do no concerted work under the distraction of the hiccoughs—to make the best of a bad situation and to study the temporal sequence of the reflex.

I timed the intervals between successive 'hics' during the 25th, 36th, and 40th hours of the attack. The results appear below.

TABLE SHOWING THE TIME INTERVALS BETWEEN SUCCESSIVE REFLEXES, THE NUMBER OF CASES OCCURRING AT EVERY INTERVAL DURING THE THREE PERIODS TIMED, THE AVERAGE TIME AND THE M.V.

Timed during the	Interval in seconds														Av.	m.v.
	Short			Medium					Long				Total			
	2	3	4	5	6	7	8	9	10	11	12	13–19	No.	Time		
25th hr.	0	1	15	27	35	75	35	25	10	5	1	0	229	25′45″	6.75″	1.55″
36th hr.	1	20	48	61	36	33	17	12	6	1	0	0	235	22′	5.62″	1.46″
40th hr.	0	2	11	27	37	60	52	22	14	5	3	8	241	29′57″	7.46″	1.62″
TOTAL	1	23	74	115	108	168	104	59	30	11	4	8	705	77′42″	6.70″	1.59″

*I planned to time the intervals during every period for half an hour, but was interrupted during the first two periods.

As the table shows, the average time of the intervals between successive reflexes varied slightly for the three different periods. During the 25th hour of the attack the reflexes occurred on the average at the rate of one every 6.75 sec.; during the 36th hour they occurred every 5.62 sec.; and during the 40th hour every 7.46 sec. Although my diaphram was sore from the prolonged spasms, the soreness did not greatly affect the rate of the reflex. The rate was, to be sure, slowest during the 40th hour period, but the difference between the 40th hour and the other periods is not great, and moreover the rate was more rapid during the 36th hour than during the 25th. The variations from the averages in all the periods is, as shown by the m.v. approximately the same. The variations are large, being in all cases about 25% of the averages.

The data of the first two periods fit, as the table also shows, nicely into a normal curve of distribution, but that of the third period—which proved to be near the end of the attack—is skewed slightly to the right and tails off in the long intervals.

An inspection of the data shows that the rate varied gradually. Short intervals tended to follow short intervals and long to follow long. In order to test this result I divided the intervals, as shown in the table, into three classes: short, medium, and long. I then averaged for each of the 'short' and the 'long' classes (1) the times of the first following intervals; (2) the times of the second following intervals; and (3) the times of the first and second following intervals. The average of the first intervals following a short interval was 5.93 ± 1.25 sec.; of the second intervals, 5.67 ± 1.40 sec.; and of the first and second intervals, 5.80 ± 1.34 sec. The average of the first intervals following a long interval was 7.79 ± 1.51 sec.; of the second intervals, 7.76 ± 1.61; and of the first and second intervals, 7.78 ± 1.56. The average of the intervals following long intervals, whether the first, second, or first and second are considered, is approximately 2 sec. longer than the intervals following short intervals.

The results of this study, which was inopportunely interrupted, show that the temporal interval between successive hiccough reflexes is, in my case, approximately 7 sec.; that there is a gradual and large variation in the rate; and that the rate is little subject to fatigue. I venture to report these results in the hope that someone will be prompted to undertake a more prolonged study. If the intervals were timed over a longer period—a week or ten days—much positive knowledge, I believe, would be gained regarding the rate and rhythm of this reflex, and the effects of fatigue upon it.

K. M. D.

Social Science Research Council's Grants-in-Aid

The Social Science Research Council announces its second annual list of Grants-in-Aid. Altogether, thirty-seven people received support. On the basis of major emphasis, their projects were distributed among the social science as follows: Anthropology 2, Economics 6, History 16, Political Science 2, Psychology 2, Sociology 5, Human Geography 2. Most of the projects, needless to say, straddle a number of these fields. In fact, while not insisting upon it, those administering Grants-in-Aid look with favor upon projects that represent a broader approach to a problem than is possible within the limits of any one discipline.

The Grants-in-Aid of special interest to our readers are: Clark L. Hull (University of Wisconsin), "To work out experimentally some of the more important phenomena of normal waking suggestion;" Carney Landis (Wesleyan University), "An investigation of the underlying factors in individual social maladjustment, particularly the possibilities of using measurements of muscular and physiological reactions as determiners of, indicators of, or criteria of individual delinquency;" Fred A. Moss (George Washington Univeristy), "Statistical analysis of five thousand tests given in industry, and ten thousand tests given in educational institutions with the aim of determining the validity of the various items in the social intelligence test, with the ultimate aim of constructing a more valuable device for measuring ability to get along with others;" A. R. Gilliland and John J. B. Morgan (Northwestern University), "A test of introversion and extroversion as part of a program of developing measures of personality."

The aid granted by the Council ranges from $200 to $2000, and in almost every case is conditional upon liberal coöperation from the institution with which the applicant is connected.

The administration of Grants-in-Aid is in the hands of a special Committee of the Council known as the Committee on Grants-in-Aid. The Committee plans to hold two meetings during the coming academic year: in mid-November and in mid-March. The closing date for receiving applications for consideration at the latter meeting is February 1, 1930. Any person who is a resident of the United States or Canada and who has evidenced real research ability is eligible. The Committee expects that the applicant's project will be well under way, that he can forecast the time and the money required to complete it, that it is not to be used in fulfillment of any higher academic degree, and that he has already canvassed carefully the possibility of aid from his own institution. Applications should be addressed to Walter R. Sharp, Secretary of the Committee on Grants-in-Aid of the Social Science Research Council, 230 Park Avenue, New York City.

M. F. W.

Deutsche Gesellschaft für Psychologie

In announcing its change of name from *Die Gesellschaft für experimentelle Psychologie* the above Society fervently pleads for a juster recognition of psychology by the German State and in institutions of higher learning. The announcement, which is signed by Karl Bühler (Vienna), William Stern (Hamburg), Narziss Ach (Göttingen), David Katz (Rostock), Johannes Lindworsky (Prague), Walter Poppelreuter (Bonn-Aachen), and Hans Volkelt (Leipzig), remarks upon recent changes of academic status in several German universities under which vacant chairs of psychology have been filled by pedagogues and philosophers. While these changes ultimately rest upon the sterility of an older and antiquated psychology, the Society contends that the subject is now vastly revised and extended and hence deserving of better academic treatment. The *apologia* notes the following modern improvements and extensions. Experimental research has been carried into the 'higher mental functions,' toward the artistic, moral and religious life, and to the subjects of character and temperament. Outside the realm of experiment have appeared the study of behavior and also social and genetic studies of man and of his products. Still maintaining its relations with the natural sciences, psychology has enriched itself by new and intimate relations with all the cultural sciences, with philosophy, phenomenology, epistemology and theory of value. Education, training, the care of dependents, and the administration of justice definitely imply the current psychology of childhood and youth. Vocation and the arts and instruments of work have been firmly placed upon a psychological foundation, while nearly every branch of medicine appropriately touches one or another phase of the same subject. German psychology, which has supplied the model and the inspiration for other lands, should not be embarrassed—as these men contend—but rather supported and cherished in its own home.

M. B.

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By J. P. GUILFORD, University of Nebraska

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